

Vol. XXIV, No. 1

PSYCHOLOGICAL REVIEW PUBLICATIONS

March, 1917

Psychological Review

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY

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JAMES R. ANGELL, UNIVERSITY OF CHICAGO (*Monographs*)

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PUBLISHED BI-MONTHLY BY

PSYCHOLOGICAL REVIEW COMPANY

41 NORTH QUEEN ST., LANCASTER, PA.

AND PRINCETON, N. J.

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879.

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WITH THE CO-OPERATION OF
MANY DISTINGUISHED PSYCHOLOGISTS

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containing original contributions only, appears bimonthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 480 pages.

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PSYCHOLOGICAL REVIEW COMPANY

Princeton, New Jersey

FOREIGN AGENTS: G. K. STECHERT & CO., London (10 Star Yard, Carey St., W. C.);
LAFITTE (Koenigstr., 37); PARIS (16, rue de Condé)

THE PSYCHOLOGICAL REVIEW

Aug 1927

THE LAWS OF RELATIVE FATIGUE¹

BY RAYMOND DODGE

Wesleyan University

The temerity that ventures to speak of fatigue laws may well arouse a critical attitude. But I shall not be quite so indiscreet as the title might be misconstrued to imply. For reasons that will presently appear in detail, I have no expectation that the laws of mental fatigue will be formulated in the immediate future. Oeffner's so-called laws of fatigue are obviously only empirical generalizations and summaries. My subject is really much less pretentious. It concerns only the relativity of fatigue. The laws of relative fatigue that we shall discuss might with equal propriety have been called the laws of fatigue relativity.

My excuse for selecting so threadbare a matter as fatigue for the subject of the presidential address is largely personal. As some of you know, I have been working on various phases of mental fatigue experimentally for a number of years—too long for self complacency. More than once it has seemed to me that I was following a clear experimental path out of the maze of fact, only to find myself back again at the starting point, facing the same fundamental questions. But however personal my interest in fatigue may be it certainly is not exceptional. I venture the guess that there is not a member of this Association but has made fatigue the subject of direct, indirect, or projected investigation. Certainly few psychological subjects have so widely interested investigators in the allied sciences. Few seem to have at once such far-reaching

¹ Address of the President before the American Psychological Association, New York Meeting, December, 1916.

bearings on psychological theory and the conduct of human affairs. Few present such a bewildering literature, with such an array of apparently mutually contradictory experimental results. None is more confused with an equal pressure for practical working rules. Confusion and eagerness for practical results make a situation fraught with grave peril to science. If anything could, they justify this attempt to clarify and systematize the fundamental concept of mental fatigue.

It would be an impracticable as well as an uncongenial task for me to attempt a review of the literature of fatigue, even if this were a fitting occasion. Our time limits, our precedents, and my personal interests persuade me rather to attempt what I hope may prove to be a more generally useful undertaking, namely, a substantive analysis of the problem. The first part of that task, as I apprehend it, is to clear the problem of some misleading assumptions by which faulty analogy and practical interests have confused the real issues. Thus simplified we shall try to redefine the psycho-physical problem on a scientific rather than on a practical basis.

Mental fatigue is one of those scientific problems that has suffered from too much practical importance. In the enormous number of investigations that have appeared since the publication of Mosso's "epoch-making" book, just a quarter of a century ago, educational, medical, and more recently social and economic interests have given the dominant motifs. It was indeed an alarming arraignment of the schools that they ruined the health and impaired the eyes of pupils by their excessive demands. An investigation of such charges was a direct obligation on experimental pedagogy. Scarcely less important than the school problems is a just determination of the proper duration of an industrial day, with a fair consideration for the welfare of the laborer and for the exigencies of competition. None of us, moreover, is entirely free from more personal practical difficulties in our desire to exploit most effectively the time and energy at our disposal. Now I would not for a moment be misunderstood to depreciate the importance of these practical studies. My only contention

with respect to them is that they all suffer more or less from an inadequate scientific basis. But in spite of all the confusion of alleged fact, all the premature and unverifiable pronouncements, most of us still believe that an adequate answer to such practical questions is both desirable and possible.

Less insistent and obvious, but none the less real and important, are the scientific problems of mental fatigue. For the present at least it may be of some advantage to keep separate the two lines of investigation, the practical and the scientific. The former is, in the main, quite independent of the latter. However the questions as to the nature and laws of mental fatigue may finally be answered, careful generalization from experience as to the expediency of certain work and relaxation sequences will deserve and will receive careful consideration in planning the day's work. So too the most advisable length and distribution of recesses may be settled purely empirically, entirely without reference to any of the underlying bio-chemical processes. For all such practical purposes the concept of fatigue is an accident. Its function is not to recall the implications that it has in bio-chemical science; but merely to serve as a vehicle for practical maxims, a class name for all sorts of unanalyzed hindrances to effective work. The hindrances would be just as real and the practical maxims just as valuable even if it were proven that fatigue had nothing to do with them. The scientific problems as to the real nature and conditions of a supposititious mental fatigue are quite independent of all such questions of practical expediency. Scientifically we must know the differential characteristics by which mental fatigue can be distinguished from the other limitations of the work curve; as well as its elementary forms and their interrelations. We must follow its implications as an indicator of the relationship between mind and body; and correlate it with other bio-chemical facts. It is these latter problems that appeal to me personally with especial emphasis. Mental fatigue if it exists in the physiological sense, must be connected in some direct way with the energy transformations in nervous tissue,

and the fundamental problems of inner psycho-physics. The great problem whether our mental life conforms like the rest of the organism to the underlying postulate of thermodynamics, the conservation of energy, must be answered if at all by the psycho-physics of work and fatigue. While I sincerely hope that adequately equipped attempts to explore these fundamental questions are not too far distant, there are related problems that can be examined by simpler techniques. Again mental fatigue if it exists ought to furnish us with an instrument of dynamic analysis of the mental complexes, reaching the inner mechanisms of our mental life. To estimate its possible usefulness, one has only to think of the analogous use of fatigue or adaptation in sense analysis; when we adapt out one sense quality and note the effect of its loss on the other qualities, or on perception in general. So, for example, the relative composition of two purples might be shown even if we had no other method, by adapting out spectral blue, and comparing their resulting appearance. It would seem that a similar process ought to be applicable to mental experiments when we have no other means of experimentally eliminating the various factors. One might even outline the working postulates of such an analysis as follows: I. Whenever in mental processes fatigue of one is regularly accompanied by fatigue of another there must be some dynamic factor common to both. II. Conversely, whenever the fatigue of one mental process does not show as fatigue of another, the two must depend on different dynamic conditions. III. Whenever fatigue in one process is accompanied by the improvement of another process then the two are probably related in the sense that the fatigued factor in the former was inhibitory to the second. That such postulates have borne little fruit hitherto, is not due to any inherent logical unsoundness, but rather to our misapprehension of the character of mental fatigue. At present their application to the problems of analysis would be handicapped by the very richness of the alleged correlations. It has proven embarrassing to more than one of us to teach our students on one occasion the very slight correlation between mental processes

that seem very much alike; and on another occasion to teach them how mental fatigue in general may be measured by the pulse, the ergograph, addition, reaction time, the dermal threshold, and other apparently disconnected events through a long list of accredited extrinsic tests. To be sure the reliability of these and other so-called tests is not universally admitted. But the gross discrepancies between genetic and dynamic correlations might well be taken a little more seriously. Before any of these scientific tasks can be undertaken with promise of success, we must know what mental fatigue really is, if there is any such thing, and how it is conditioned.

THE CONCEPT OF MENTAL FATIGUE

The concept of mental fatigue is so familiar that a precise analysis of its differentia has seldom seemed necessary. Statements of its meaning, when they occur, regularly emphasize a diminution of some product of mental activity per unit of time, incident to continued activity, and as Thorndike insists, recoverable through rest. Actual recovery or the capacity to recover through rest seems to me to be irrelevant. On the one hand it excludes extreme fatigue; on the other hand it fails to exclude all sorts of intercurrent disturbances. But the diminution of production consequent to continuous work with or without recoverability, is I believe an untenable criterion of fatigue.

If the word fatigue has any scientific propriety in connection with our mental life, it seems to me that it should refer to the metabolic conditions of mental action, not to any predetermined characteristic of its consequences. This is very much the same point that I made recently concerning mental work. While that was not received with the unanimity that I had hoped, in the case of fatigue at least, failure to realize the dynamic implications must lead to gross confusion. Obviously psychology or pedagogy is entirely competent to ignore the physiological concept of fatigue and to develop its own empirical concept as decreased returns of mental work. But if it ignores the metabolic implications at the beginning, it may not assume at the end that physio-

logical and pathological fatigue processes parallel the decreased returns. It is such gratuitous assumptions concerning matters of fact that make psycho-physical parallelism a dangerous working hypothesis. Moreover, such an independent psychological concept would be scientifically defensible only to the degree that work decrements, consequent to work and eliminated by rest, prove to be homologous processes with regular and definable antecedents. If on the contrary work decrements show a large variety of types, or follow any considerable variety of conditions, it would seem to be good sense and sound science to enquire whether any of the varieties of mental work decrement correspond to physiological fatigue processes. These alone would then seem to have a natural right to the name mental fatigue. From this standpoint other decrements would be regarded as pseudo-fatigues.

In order to conserve our time let me be quite direct and frank. I regard it as improbable that any of the mental work decrements so commonly treated as mental fatigue, are ever simply conditioned by true fatigue processes in nervous tissue. Conversely real fatigue may not appear as a decrement at all. Some of the evidence for this position can only be indicated here. Some of it must be given in more detail.

First one must note the physiological fact that nervous tissue *in situ* has been found quite resistant to fatigue and exhaustion under normal circumstances. The axis cylinders apparently never fatigue except under experimental conditions when their environment is freed from oxygen, or when they are narcotized so that they are unable to use the oxygen that is present. Cell bodies are likewise resistant to fatigue under normal circumstances. They can be exhausted in experimental animals only under strychnine poisoning, after the withdrawal of normal blood supply. Langfeld has shown that in humans prolonged fasting produces no correlated decrease of neural efficiency. Reflexes like the knee-jerk and the protective lid reflex show no decrement after long series of elicitation, if care be taken to prevent intercurrent general depression of the nervous system. In those cases where

fatigue decrement of the reflexes does occur, there is evidence that neither the muscles, the nerves, nor the nervous centers have lost their irritability except to the particular stimulus to which they have become adapted. On the contrary hyper-excitability is a common if not a regular phenomenon of extreme so-called mental fatigue. At any rate it would seem that the complete cessation of mental processes, like the inability to recall an opposite, to complete a sentence, to recite a series of nonsense syllables, or to multiply four-place numbers by mental arithmetic cannot possibly mean the fatigue of nervous tissue to the corresponding degree of completeness.

A second ground against the traditional differentiae of fatigue is their failure to exclude normal psycho-physical rhythms. In more than one respect it was an unfortunate accident that the paradigm for the interpretation of the phenomena of mental fatigue was the fatigue of a nerve-muscle preparation. Undoubtedly there are many and important analogies between the action of lower spinal arcs and cerebral processes. But after all the main task of physiological psychology begins when it seeks to understand the differences between the simpler processes and cerebral action. Similarly, in connection with a supposititious mental fatigue the regularly increasing work-paralysis of nerve-muscle preparation may be and in some respects must be a misleading model. One of the great differences is that while the extirpated preparation changes only slowly under experimental conditions when unstimulated, normal mental life precludes unchanging neural conditions. In the complex interconnections of human cortical processes the one statement that can be made with completest conviction is that the experimental subject never remains constant, quite apart from the intended experimental changes. Even under the best possible experimental conditions, the experimental change is only one of the changes that we know to be occurring. The constitution of these non-experimental changes in any given case we know only in part. We believe that consciousness itself is a process which involves more or less continuous inherent

change. We know that there are also various intercurrent physiological rhythms, cardiac, vascular, respiratory, intestinal, glandular, and muscular. Cortical action may also initiate non-rhythmic changes in the glandular, circulatory, and respiratory systems with far-reaching reactions of those changes on the cortical action that originated them.

From the standpoint of the importance of the accompanying mental changes, perhaps the most significant of the rhythms is sleep. The fatigue-hunting enthusiasm that finds in sleep the daily climax of fatigue is without physiological justification. On the contrary, we have learned from experimental investigations that for some persons evening may be the time of most effective mental work. Moreover, it is neurological commonplace that in serious extreme fatigue, sleep may be impossible. Physiologists would welcome any insight that we could give them into the causes of sleep. The fatigue climax assumption simply is not tenable. Whatever they may be, we know that the conditions of sleep are not simple. Habit, the absence of stimuli, probably widespread inhibitions, and possibly gland products and vaso-motor changes coöperate in its production. Sleep may come from restriction of activity quicker than from over-exertion. Lecturers never go to sleep. The audience may. In view of such complication of the conditions of continuous work decrement the assumption that all diminished returns consequent to work and eliminated by rest are fatigue seems to me utterly untenable.

A third ground of suspicion against the true fatigue character of most so-called mental fatigue is found in the means that are commonly used to induce it. In nerve-muscle fatigue experiments one isolates a specific tissue and stimulates it successively in the same manner. In mental fatigue experiments, on the contrary, repetition of the same stimulus is systematically avoided. The more carefully one analyses the assumptions of this anomalous technique, the more incongruous it appears. Let us take a concrete instance from what Thorndike has taught us to regard as one of the purest forms of mental work, mental arithmetic. If we strictly

followed the analogy of physiological fatigue experiments some association in mental arithmetic, say the multiplication of two times two, should be repeated until work decrement or paralysis indicated fatigue of the association process. As far as I know that is never done. It seems absurd. The experimental device of constantly changing the stimulus in fatigue experiments is defensible only on the assumption that all multiplication processes affect the same general group of tissues, and that continuous multiplication of different digits increases the sum of the fatigue of the whole. But neurologically the assumption is certainly a strange one that the nervous tissue which was involved in one association fatigued more when a variety of different associations were made than it did when all the burden fell on the same associative elements, operating continuously or in rapid succession. Moreover, there are no facts available to show that restriction to a single field like multiplication will produce greater work decrement than rapid change from one field to another. On the contrary there is evidence that the greater the complexity of the mental task the more pronounced is the decrement. Such decrement, however, is more probably due to a confusion between different paths of discharge than to fatigue of any particular path. That confusion is real and a common experience every introspective account is evidence. Theoretically it should be expected from the operation of the known laws of association. Suppose, for example, that after adding various digits to seven we come to the task of adding four. The right associate is by hypothesis well known and thoroughly practiced. But if other numbers have recently appeared in the series they also tend to be reproduced on the basis of recency. It is at least conceivable that the true associate in such a case might be difficult to recall, not at all from fatigue of the corresponding tissue but from effectual inhibition because a more recent associate appears in its stead. The necessity for inhibiting irrelevant and false associates is certainly a common experience in the elementary mathematics of some of us. But the tendency of recent ideas to recur is not in any sense a fatigue or exhaustion

process, but is probably a matter of residual excitation and summation. Such work decrement then is not fatigue but mere association rivalry.

A fourth ground against identifying work decrement and fatigue may be found in the operation of incidental inhibitions. Theoretically, every mental operation arouses more or less widespread associated reverberations which manifest themselves in the sequences of actual associative recall, and may on occasion, as we have just seen, operate to confuse the regular sequence of work by a kind of associative rivalry. Theoretically also, every actual association process involves more or less widespread inhibitions of undesirable associations. Now it is conceivable that these useful inhibitions of the irrelevant might operate to produce a pseudo-fatigue work decrement in any extrinsic test. For example, I have published experimental evidence that the most intense mental work of an examination period commonly follows the first reading of the examination questions. It is the period of adjustment to the examination as a whole, when widespread association systems are being organized. Such activities are not possible in fullest degree without corresponding inhibitions. Ordinarily distracting stimuli pass unnoticed. Even physical discomfort and pain may for a time be ignored. Now it is conceivable that if at such a time the fatigue tester should request the examinee to add digits for two minutes as rapidly as possible, the response might show a degree of work decrement that bordered on total incapacity. Or again, suppose we would measure the fatigue of a Wall St. broker, hour by hour, with the æsthesiometer test. And supposing as the hour struck we should interrupt a selling campaign that was taxing his skill as a broker by the request that he submit to our compass point test. The chances are in favor of some rather vigorous verbal defensive reactions with no discrimination at all. But if we were able to hold him to a promise and actually start the test, is there any guarantee that gross decrements in the measured function, all due to previous work and remediable by rest, might not be due to his inability to give his attention to our petty tests while his fortune was at

stake on the floor? Of course the whole situation is absurd. The most enthusiastic believer in space threshold tests would hesitate to use such results as an indication of the broker's general mental fatigue at that time.

We freely admit that these are extreme cases, and that they break the most elementary rules for experimentation. But have we any guarantee that similar discriminations against some seemingly unimportant task might not occur just after recess, or just before school lets out, when the afternoon's escapades are in the making, or any time at the interruption of seemingly important processes? Conversely is there any guarantee that the interruption of annoying or even fatiguing work by a few moments of trivial testing might not be a joyous relief, giving results that might entirely hide a supposititious real mental fatigue of the interrupted work? I am not arguing that such inhibitions would not be very much worth knowing; but merely that it confuses their real bearings to call them all fatigue.

In addition to these specific inhibitory processes which are commonly classed in psychology as phenomena of attention, we are acquainted with secondary inhibitions through a diminution of the supporting organic processes, glandular or circulatory. Of the glandular changes I have no direct knowledge. The initial increased pulse frequency, whenever complete relaxation is interrupted by any mental activity, is commonly followed by a gradually decreasing heart rate in any prolonged experimental task. We may regard this as a kind of adaptive process, an habituation to the task at hand. It is difficult to conceive of it without reference to the gradual elimination of extrinsic excitations, in which an initial general excitation is followed by an inhibition which restricts the excitement to selected processes. I have been able to demonstrate that something of this sort occurs in every normal reading pause. That continuous fixation of a trivial object is inhibitory is shown by its familiar hypnagogic tendencies. It is one of the methods of producing hypnosis. With some probability we can predict a diminution in the organic conditions for metabolism in all relatively unused neural centers.

during monotonous mental work. In extreme cases continuous disuse leads to atrophy, muscular, neural, and glandular. To regard work decrement which is due to more or less complete atrophy of unused paths as fatigue would be a manifestly absurd confusion of concepts. But work decrement from secondary trophic deficiency, as in unused parts, is just as surely not fatigue. Just as in periods of excitement and important readjustment, there are undoubtedly vascular and glandular changes which increase the activity of the whole neural mechanism, reflexly reinforcing the processes that initiated them; so it is probable that general depressions of glandular or vascular origin accompany monotonous mental work, in which even the centers that are most active finally participate. But this again is not fatigue in any physiological sense.

In as far as these various processes represent work decrements or decreased returns that might be mistaken for neural fatigue they may properly be called pseudo-fatigues. We have described pseudo-fatigues of intercurrent rhythms, of residual excitation and rivalry, and of specific and trophic inhibition. The pathological evidence that work decrement is no true indicator of nervous fatigue is not new. Even to summarize it would extend our paper too far. But I think that without it, we have established the thesis that decreased returns resulting from work and recoverable by rest if you will, cannot be employed as simply and directly in the higher neural systematizations as it can in simpler tissues. Arbitrarily to define mental fatigue as work decrement is effectual self-banishment from physiological tradition as well as from clearly defined fields of investigation of the utmost importance.

Having divested the mental fatigue concept of its irrelevant content as vehicle of the various work decrements, it is now in order to inquire whether there is in our mental life a real fatigue phenomenon. I believe that there is, but its manifestations differ from the paradigm of nerve-muscle fatigue in two important particulars. These are: first, the inconstancy of the stimuli in mental work; and second, the

interaction of competing paths. These two differences combine to emphasize the relativity of all mental fatigue.

THE RELATIVITY OF FATIGUE

In the nerve-muscle fatigue experiment, the stimulus is always simple, and usually constant in intensity, given at regular time intervals. For a variety of reasons the stimulus that is most used is the faradic current. It is capable of fine adjustment, may be held at constant intensity over long periods, and is exceedingly effective in quantities that do not damage the tissue. No physiologist would start a fatigue experiment with stimuli of unknown and variable intensity. Unfortunately, that seems to be the only practicable method at present in so-called mental fatigue experiments. Nobody knows the relative stimulus value of two different mathematical sums. But what is vastly more embarrassing, nobody knows how to follow or to evaluate the ever-changing inner factors in the total mental stimulus, such as the force of the instructions, the personal interest of the subject in the scientific aspect of his task, in its bearing on the particular exigencies of his academic career, and so forth. It was one of the great services of Kraepelin in his analysis of the work curve to show how these inner stimuli may change during an experimental period. The meaning of that analysis as I apprehend it is not given in the precise variables or spurts that he found, nor in the assumption that they are always present, but rather in the demonstration that variables in the inner stimuli may occur and must be reckoned with. It would not take us long to add to his objectively defined list many others taken from our experimental experience, such as competition and personal pride, repetition of the instructions, encouragement and persuasion, the presence of the instructor, rewards and penalties of various sorts, and the unanalyzed mass of obligations.

I am not unaware that this matter of the inner stimuli to mental work is packed with problems that we have no adequate techniques to investigate. But that is no excuse for ignoring them. It is our business as scientists to try to

see things as they are, even if they are complex. There is at least some ground for the suspicion that most if not all our real mental fatigue of the work decrement type is really a fatigue of the inner stimuli rather than of the capacity to react. This at least would account for the extraordinary correlations in the fatigue of the most diverse functions. In many so-called mental fatigue experiments the only common factor discernible to introspective analysis is the intent to keep working as fast as possible to the neglect of competing interests.

Now in the physiological experiment fatigue may be shown in two ways, either by a rising threshold or by decreased response to a constant supra-threshold stimulus. Only in the latter case is there an obvious work decrement. The former case implies a constant work output with a gradually increasing stimulus intensity. In mental work we are often distinctly aware of similar changes in the intensity of the inner stimuli that keep us at a disagreeable or monotonous task. Mere interest in the task may lose its force comparatively early. Then the task is continued from stubbornness, the dislike to fail, sense of obligation, honor, fear of ridicule, or hope of reward, etc. All of these may operate in succession. In the end all of them may lose their force and we say, "I do not care what happens, I cannot go on with this thing any longer to-night." There may have been no important work decrement until the break, as Yoakum calls it. But the process is none the less a real fatigue if the continuation of work depends on a change of the stimuli.

All of this emphasis on the importance of the neglected factor of changing stimuli in the fatigue concept is probably sufficient to justify the formal statement of a necessary correction in the traditional definition of mental fatigue. We may call it the first law of relative fatigue, neglected rather than new. Without pretending to give it final formulation we may express it as follows: Within physiological limits, all fatigue decrement in the results of work is relative to the intensity of the stimulus.

Education and society have a very practical interest in

this phase of the fatigue problem. They make use of a large number of incentives in which as Thorndike wisely points out the changes in satisfyingness may be a real cause of work decrement. The adequate adjustment of stimuli to the development of the individual and the needs of the case would seem to be a very real problem in the training of backward and gifted as well as normal children. It seems strange that we have so little experimental knowledge of the relative value of available reinforcements. Autogenic reinforcement is, I believe, at least one factor in the underlying psycho-physics of James's 'reservoirs of power' which may be quite as significant for psychology as the action of adrenin to which Cannon has introduced us. That continuous activity under the reinforcement of emotion or even in the educational use of play may be a source of serious fatigue we have been warned by Kraepelin. Some other reinforcements are conspicuous for their insistence. Such a one is worry. It would seem to be no accident that this is so closely connected with exhaustion psychoses.

I believe that the relative value of the various inner stimuli would repay the closest study. Just now it seems to be interesting the abnormal rather than the normal psychologist. Practical experience is full of rough approximations. Their refinement by experimental techniques would not seem to be an impossible task.

It is possible that we can study relative fatigue not merely by the changes that occur during long series of repetitions but more expeditiously in the relative refractory phase which the genius of Verworn proved to be identical with the fatigue process. Since the relative refractory phase is common to all nervous tissue, I have asked the question whether we can find in mental processes a similar phenomenon. This is undoubtedly the case. In fact every mental process shows something analogous. Repetitions of all sorts seem to be avoided whenever practicable. The repetition of questions, courses, lectures, phrases, and even words is possible enough, but except for special reinforcing circumstances, it is postponed until the effect of the initial case is somewhat worn off.

The routine is regularly less alluring than the unusual. Mankind in general prefers new scenes, new plays, new walks, new jokes, new styles, new investigations. Possibly the decreased effectiveness of over-memorization is a case in point. Possibly even the loss of attention to frequently repeated processes, which is commonly regarded teleologically as a freeing of consciousness for new adjustments, may be caused by the longer refractory phase of the more complex systematizations of attention, so that the rapidly repeated task is dynamically excluded from conscious emphasis.

Works of art on the contrary are characteristically resistant to the refractory phase. Possibly this results in some way from their origin. Certainly one of the marks of good art is the constancy of its appeals. The popular song, the clever phrase, the good joke, soon finds us refractory to the point of desperation, though it is notable that we become refractory to their reception much quicker than to their execution. We like to tell old jokes better than to hear them. But the great classics in music and literature may be heard over and over with increasing satisfaction. It is not impossible that Aristotle's catharsis by dramatic representation of suffering and evil really operates by developing a refractory phase, and a kind of relative fatigue. How far this principle operates in habituation to environment, indifference to shocking conditions of poverty and morals, to suffering, and to the horrors of war, as well as to luxuries "when the novelty has worn off," I am not prepared to estimate.

It would seem that some of these or analogous phenomena ought to yield data for a scientific study of the intensity of the inner stimuli in connection with fatigue if we only knew how to use them. But the very difficulties of technique emphasize how far we are from a real knowledge of relative mental fatigue.

The simplicity of the nerve-muscle paradigm of mental fatigue is further misleading in that it gives no indication of certain important complications which are characteristic of higher systematizations, and which Sherrington called their competition. In a nerve-muscle preparation the impulse has only one possible path. In the higher nervous system on the

contrary any afferent impulse may theoretically activate every efferent path. Just which motor process it finally initiates, is determined by a kind of competition. Competition appears in the spinal reflexes though less conspicuously than in cortically conditioned action, where it is the rule. Unfortunately, however, just where it is most significant it can seldom be followed objectively by our present means of investigation. But there are clear evidences of its operation in associative thinking, in attention, and in perception as well as in conduct.

The relatively fixed tendencies of competition in the cord are probably determined very simply by neural growth and development. In higher systematizations the outcome of competition tends to follow habitual patterns which have originated in the varying life-history of the several competitors. At any given moment in a developed system of this sort, the outcome may be modified by a variety of reinforcing and inhibiting accidents. Among the latter we must count fatigue. In closely balanced competition the absolute degree of fatigue need not be high to make it a deciding factor. Indeed it is conceivable that if the balance of the other factors is close enough, an infinitesimal fatigue, or the slightest trace of a refractory phase may totally change the character of the response, just as intrinsically trivial reinforcements or accidental inhibitions may be the arbiters.

This relation of fatigue to balanced competition gives us a second type of fatigue relativity. Fatigue is relative, not only in the relation of apparent work decrement to stimulus, as expressed in our first law; it is also relative in the sense of a proportionate fatigue of the various factors in a competing group. We may tentatively express this second type of fatigue relativity in the form of a law which for want of a better name we may call the Second Law of Relative Fatigue, because it implies a higher systematization than the first law. In any complex of competing tendencies the relatively greater fatigue of one tendency will tend to eliminate it from the competition in favor of the less fatigued tendencies.

Unfortunately the mechanism of competition cannot be

studied at all in simple preparations and only imperfectly in the reflexes. The most characteristic systems are the least accessible. In the search for accessible human systems of greater complexity than the reflexes, it occurred to me, something over ten years ago, that the motor apparatus of the eyes offered some unique advantages. There we may study twelve intimately related and delicately adjusted final paths which are directly connected with reactions of considerable biological importance. Furthermore, every variation of their interaction is capable of being recorded on a single plane, without complicated mechanical devices, and without the distortions incident to the moving of heavy masses, like the limbs. Since that time the eye-movements have proved to be unusually valuable indicators of neural conditions in some forms of insanity and under the action of alcohol. In experiments that are now in progress they give promise of being the most consistent indicators of general neural conditions. In the early hopes of using them for an analysis of fatigue phenomena, I took a considerable number of binocular records of rapid successions of eye-movements after the model of the ergograph. Though reported on informally from time to time these records have never been published before because of my inability to account for some of their most conspicuous peculiarities. As these difficulties have gradually been experimentally cleared, the records have been seen to illustrate in a remarkable way some of the characteristic phenomena of mental fatigue, and pseudo-fatigue. In particular they admirably schematize the second law of relative fatigue and the "breaks" that it conditions.

Let me assume your familiarity with the technique of photographically recording the eye-movements from the corneal reflection. For the present records the eyes moved horizontally through an arc of sixty degrees, fixating successively two knitting needles which were situated thirty degrees on either side of the primary position of the line of regard. Each dot or dash on the records represents one phase of the alternating current, and a time interval of about eight thousandths of a second.



FIG. 1.

FIG. 2.

The succession of eye-movements in the records that are here reproduced was as rapid as practicable with subjectively adequate successive fixation of the two fixation marks. Some of the more characteristic fatigue phenomena which they show are: (1) The speed of movement becomes less towards the end of the series; (2) the fixations become less accurate; (3) and finally the line of movement itself becomes more irregular. Fig. 2 shows the climax of these processes in a break. The gradual decrease in angle velocity corresponds to the work decrement of extirpated muscle. But in this case, in view of Sherrington's demonstration of the reciprocal inhibition of antagonistic eye-muscles, it doubtless involves something more. The greatest angle velocity of eye-movement could only occur when the relaxation of the antagonistic was perfectly coördinated with the contraction of the agonistic muscle. The pseudo-work-decrement in this case then is not purely muscular but is in part a matter of defective coördination. The increasing errors of coördination have a similar origin. That is, the total elaboration of the contraction impulse and the corresponding relaxation of the antagonistic becomes less exact in successive repetitions of the act of fixation. But the coördination is not limited to the internal and external recti as one might expect them to be in horizontal movements of the eyes. All the records of 60" eye-movements, which I have ever seen, show a vertical factor. In all my records this vertical factor results in an elevation of the line of regard. But it varies from movement to movement. That these vertical components are not accidents of purely muscular origin is shown by binocular records. Since the disturbances are homologous for both eyes, their origin must lie in the central nervous system. While occasional gross disturbances occur early in the series of movements, they become more and more conspicuous as the series progresses. The vertical components represent the intercurrent action of related and competing, but this is a case of non-inhibiting systems. When they become extreme they tend to interrupt for a moment the main rhythm of horizontal movements. In some cases these various disturb-

ances produce a moment of confusion and a break in the process, which in ordinary mental fatigue experiments would be interpreted as complete fatigue or exhaustion.

Our eye-movement schema for the relative fatigue of competing systems is particularly free from complications through extrinsic facilitations and inhibitions. Retinal fatigue or adaptation is reduced to a minimum by the eye-movement itself, and the consequent shift of the area of stimulation. The homologous fixation marks, under constant illumination present the same stimulus for each reaction in the same direction. Cortical conditions of the successive reactions, such as interest, attention and motives to continue at work, cannot of course be guaranteed to remain constant. But the experiment itself introduces no obvious distractions like the physical discomfort of the ergograph. Moreover, all our relative fatigue phenomena appear during short experimental periods.

In order to protect our conclusions from the dangers inherent in a single line of experimental evidence, I sought other similarly complicated coördination systems. While thus far no other has been found with all the advantages of the horizontal eye-movements, those movements of the index finger which Bergström recommended for ergographic work show a similar complication. Undoubtedly the strongest and best practiced oscillatory movements of the index finger are the flexion-extension movements. Considerably less easy for most of us are movements of the finger sideways in the plane of the hand. In any event, rapid oscillation of the finger in this direction is always disturbed by intercurrent action of the flexors and extensors. Their action prevents rectilinear movement, decreases the angle velocity, and finally may so confuse the process as to produce a break in the sequence of oscillations, quite like the disturbances of the eye-movements.

It was the phenomena of these relatively accessible complex systematizations that forced me to a re-analysis of the mental fatigue concept. I believe that our eye-movement paradigm gives us the clue not only for a more intelligent experimental investigation of mental fatigue, but also for the

interpretation of previous investigations. The very irregularity of the traditional results may be an expression of the laws of relativity. But I hope that the time has passed when an experimenter will be content to give us only the work decrement as datum for the measure of fatigue. Certainly the break can no longer be regarded as a temporary exhaustion of a function. Perhaps the least expected change that the new paradigm will make in our tradition is the place of the interfering sensations of weariness. These may, after all, turn out to be subjective indicators of real fatigue. Their effect in apparent work decrement, however, will be determined by their relative importance in the group of competing tendencies. Under normal conditions at least I doubt if we should call weariness a pseudo-fatigue.

It will be noted that the eye-movement paradigm is still much too simple to apply directly to our mental processes. In place of its anatomically restricted competition to the nuclei of the third, fourth, and sixth cranial nerves, we have reason to believe that cortical competitions are as indefinitely complicated as the various active association tendencies. That a variety of tendencies to associative reproduction are normally aroused as the effect of a mental stimulus is indicated by the facts of the association experiment. This normal spread of excitation, coupled with the effect of psychophysiological rhythms, and the complication of simultaneous stimuli from the different receptor fields, gives the competition in mental operations an almost chaotic complexity. But in addition to all that, we must extend our notion of competition and relative fatigue to those more slowly changing inner determinants of action that we call motives, controls, and the like. Indeed it seems probable that these inner factors, in so far as they are the only continuously acting factors in mental work, are more apt to be the locus of absolute fatigue than the several discrete association tendencies which are involved only occasionally in the mental task.

But aside from the obvious differences in complexity our paradigm adequately represents the fundamental processes. However long a mental process may be continued and how-

ever insignificant the decrement in returns, there comes a moment when it stops. It may be interrupted by demands for food, for sleep, or by some competing task. It may be interrupted by the gradually increasing insistence of inhibiting sensations like thirst, eye-strain, muscle pains, or pressure pains from sitting still. In any case, the work decrement of the consequent break can never be fully understood if we regard it as a direct product of fatigue, but only in connection with the intercurrent competing tendencies. Fatigue may be a contributing factor, but the apparent decrement of the break will bear no regular relation to the degree of absolute fatigue in the tissues which performed the discontinued task.

This enables us to understand why in pathogenic nervous exhaustion, the physician in search of a therapeutic measure may seek to strengthen some competing interest. He may even try to develop some fad, philanthropy, golf, the calculation of food calories, or what not, to compete with the old system and its emotional, business, or religious reinforcements. Most normal lives seem too full of competing tendencies. In my own case I have been interested in observing how every prolonged period of monotonous work like correcting papers, for example, finds before its close some insistent demand for interruption. If I successfully suppress one demand, more insistent ones arise, until finally effective voluntary reinforcement of the main task suddenly ends. The voluntary reinforcements may have developed such sensations of strain that the surrender to a competing impulse brings great relief. I know that the interruption is not permanent. I consent to it to get the lesser matter off my mind, expecting to return presently to the main task, freed from the incubus of that particular competitor. In very much the same way, after lying awake for a time on one side we turn over, not because we could not lie on that side longer, not because we expect any great improvement from the change, certainly not because we expect to lie on the other side forever. The displacement of the body mass is scarcely the product of fatigue. But in the complex of competing tendencies a little relative fatigue becomes the occasion for an

entirely disproportionate result. Possibly social unrest follows a similar course. They seek a change in the government, or the social and labor conditions, not because the present is really unendurable, not because they expect a permanent betterment. In many cases at least, they act from relative fatigue, to shift the pressure. I suppose all the phenomena of restlessness and the corresponding attractiveness of change finally reduce to competition and the relative refractory phase. They operate in work and play, in social and economic activities, in politics and in religion. Without their interference in our lives, unwelcome as it often is, we must have continued indefinitely in the direction of our first activity, with the consequent loss of that vital equilibrium on which the organism as a unit of different parts depends for its continued existence. Without their interference the initial process must always work itself out to the final collapse of complete exhaustion.

Relative fatigue, then, is not a mere limitation of human efficiency. It is not exhaustion, but prevents it. It is a conservator of organic equilibrium, as well as a condition of organic development. The incapacity of the young child for long-continued monotonous tasks may be a symptom of an active, developing mind. Lack of competition would result in mental deformity, or absolute exhaustion, just as truly as the lack of stable reinforcing systems in the adult would mean perpetual infantilism. Thus it seems to me that the principles of relative fatigue have a direct bearing on the practical problems of education which the traditional doctrine of fatigue as apparent work decrement entirely missed. The development of the capacity to sit still, to continue long at routine work, the adequate response to all formal discipline demands more than the strengthening of the corresponding neural bonds. It demands the weakening or elimination of competing tendencies. At least one of the perils of routine education arises from this depression of spontaneity. But I have expressly disclaimed any right or intent to discuss the practical side of the problem.

I cannot quite resist the temptation, however, in closing,

to point a methodological moral. There has seemed to me to be something almost humiliating in the eagerness with which tests of mental fatigue have been sought, while there is still so much that is uncertain in our knowledge of the fundamental nature of the process that we would test. If it is not too great a strain on presidential license at a meeting like this, when the program is so largely devoted to the matter of tests, I would sound a note of warning that in my opinion any tendency to supplant psychological investigation by tests would contain a serious menace to the future of psychology. Both have their proper place. But it can only lead to confusion and work to the discredit of our science if the search for practical tests blinds us to the necessity for studying the dynamics of the processes that we hope to test. We cannot afford to develop a new phrenology.

MORE CONCERNING THE TEMPORAL RELATIONS OF MEANING AND IMAGERY

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The controversy between the imageless thought adherents and their opponents has been lent a new aspect by the work of Dr. T. V. Moore.¹ He has ingeniously devised a simple but most fruitful method for investigating the temporal relations of meaning and imagery. By the use of this method, he has brought forward striking evidence which seems to support the contentions of the imageless thought school. The present investigation makes use of Dr. Moore's method, but by applying it to a greater number of subjects has obtained data which point to a qualification, if not a contradiction, of Dr. Moore's conclusions.

In the part of Dr. Moore's work which directly concerns us, he presented to his subjects the names of easily visualizable common objects, such as Zange, Fernglas, Pfeil, Messer, Lampe, etc.,² and asked them to react according to either one of two instructions. One time he would instruct them to react just as soon as they had obtained the meaning of the word, another time just as soon as they had obtained a visual image of the object which the word named. Nonsense words were occasionally introduced as a check to make sure that the subject was reacting to real meanings. By averaging up the times separately for the two kinds of reaction, he found whether, on the average, the subject could obtain meaning or visual image in shorter time.

Of the 9 subjects to whom he applied the method, all but one gave unambiguously shorter average reaction times for meaning than for visual image. In the case of the one,

¹ T. V. Moore, 'The Temporal Relations of Meaning and Imagery,' *PSYCHOL. REV.*, 1915, 22, 177-225.

² The work was performed at Prof. Külpe's laboratory in Germany.

the figures showed a tendency in the opposite direction. The average reaction times were slightly shorter for visual image than for meaning. The trustworthiness of these figures was, however, called into question by the fact that this subject was never cured of a habit of reacting to nonsense words as readily as to real words. This fact led Moore to reject the figures of this subject as inconclusive, and to draw all his conclusions from the results of the 8 subjects who agreed.

Introspections were asked for after each reaction and it was found that the introspection of these subjects bore out the testimony of their objective reaction times. They all agreed, that is, in reporting an awareness of simple meaning which appeared in every case prior to the image. Further introspection indicated that this awareness of meaning was a totally different kind of content from image.

From these facts, combined with similar ones obtained from experiments on the time relation of meaning and *verbal* imagery, in which pictures of objects were shown the subject, and he was sometimes instructed to react when he obtained the meaning of the picture, and sometimes when he obtained a verbal image of the name of the object, Dr. Moore concludes that meaning as a psychological content is *sui generis* and independent of imagery.

The present writer was led to question these conclusions because of the conviction that his own consciousness of meaning depended in no small part upon visual imagery. With that conviction in mind, he attempted by a method essentially similar to Dr. Moore's (to be described below) to put the matter to test. Great was his surprise, however, to discover that he himself, when the experimental conditions were thus controlled, substantiated the results of Dr. Moore's subjects, in that, on the average, he obtained meaning in less time than he did visual image. In the course of more or less haphazard experimenting, however, the writer, largely by accident, discovered a subject who *did* fulfill the prediction he had made for himself; a subject, namely, who obtained visual image, on the average, in as short a time as she ob-

tained meaning, and who declared that introspectively the image was a part of, or essential to, the meaning.

This purely chance discovery suggested to the writer that, if a large enough number of subjects were to be examined, a small proportion might be found whose results would agree with those of the subject just mentioned rather than with those both of the writer himself and of Dr. Moore's 8 subjects. In pursuance of such a possibility, an investigation was undertaken of as many Northwestern University students as possible who were at the time taking either the introductory course in psychology or the laboratory training course.

The method employed was slightly different from that used by Dr. Moore. Instead of presenting a purely chance list of names, names of black or of white objects only were presented. The subject was given two keys, one for the right hand and one for the left, and instructed to react with the right hand if the object were black, and with the left hand if the object were white. The following typewritten instructions, which were read by the subject before the beginning of the experiment, will make the method clearer:

"You will either be shown the name of something which is black or of something which is white. If it is black, you are to press the right-hand key; if white, the left-hand one. Sometimes you will be told beforehand that you are to react (*i. e.*, press the appropriate key) the instant you *know* whether the object is black or white, irrespective of how you know it. Other times you will be told beforehand that you are to react the instant you *see from your visual image* whether the object is black or white. Introspection will be asked for from time to time during the course of the experiment."

Each word was typewritten on a slip of paper which could be fastened to a piece of black cardboard; the latter was cut so as to slip into place directly behind a pair of shutters. These were made to swing open towards the subject by means of a camera-bulb. When opened, they exposed a black field in the center of which appeared the slip of paper with the typewritten word.

A Bergström pendulum chronoscope was used.¹ This was

¹ Described as Model No. 2 in *PSYCHOL. REV.*, 1910, 17, 1-18.

arranged so that the opening of the shutters closed a circuit which by means of a magnet released the pendulum. The pressing of either reaction key closed another circuit, which by means of another magnet stopped the pointer carried by the pendulum. The scale over which the pointer passed was calibrated to be read directly to thousandths of a second. The complete swing of the pendulum lasted 2 seconds only; any time longer than 2 seconds could not, therefore, be recorded.

The chronoscope was tested by means of a seconds pendulum before the beginning of the investigation, and the strength of the currents in the two magnet circuits were found such that a complete swing on the seconds pendulum registered on the chronoscope correctly to within 0.005 of a second. These strengths of current were noted and established throughout the course of the experiments by means of rheostats which were included in each of the two circuits.

Each subject was presented the same list of 24 words. The reaction times of the first 4 were rejected. The remaining 20, the times for which were counted, were the following: coffee, lime, steam, snow, mud, coal, swan, iron, diamond, crow, plaster, negro, cinders, raven, milk, print, teeth, jet, cement, lard.

It will be observed that 10 of them are names of white objects, and 10 names of black objects.

The programme was arranged so that for 5 of the 'white' words the subject was instructed to react to meaning, and for 5 instructed to react to image; the same held for the 'black' words. But different sets of 5 were used for meaning and image, respectively, in presenting this same list to successive subjects.

The testing of each subject took about 30 minutes, and 49 subjects in all were tested.¹ In Table I. we present the final results for all the subjects.

¹ The writer wishes to express his indebtedness to Mr. Leslie B. Bunch and to Mr. Wilbert C. Keiser, who helped as experimenters throughout this part of the investigation, and to the latter also for his assistance in the preliminary experiments and in setting up the apparatus.

If, for any reason, there was a slip on the part either of the experimenter or of the subject, so that no reaction was obtained for one of these words, this was usually rectified by making the subject react in the same way to another word of the proper color at the end of list.

TABLE I

Men			Women		
Subject	M.	V. I.	Subject	M.	V. I.
Mr. Bark.....	.823	1.385 ¹	Miss C.....	.742	.752
" Bu.....	1.199	1.784 ¹	" E.....	1.378	1.888 ¹
" Ca.....	.641	1.014	" Go.....	.952	1.056
" Co.....	.617	.684	" Gr.....	.778	.944
" De.....	.868	1.039	" Gu.....	Could not obtain visual images.	
" Dy.....	.796	1.051	" H.....	.834	.942
" E.....	.764	1.027	" Ed. J.....	.983	1.266
" M.....	.794	.954	" El. J.....	1.058	1.145
" N.....	1.524 ²	2.000 ³	" S. J.....	.892	1.266
" O.....	.887	1.289	" Ka.....	1.154	1.318
" Pa.....	.843	.932	" Ll.....	1.003	1.247
" Ril.....	.884	.918	" Mas.....	.756	.770
" Rit.....	.792	1.047	" Mi.....	.654	.841
" Si.....	.617	1.044	" Pa.....	.916	1.335
" St.....	1.233	1.457	" Pe.....	1.193	1.973 ¹
" T.....	.664	.932	" Po.....	.853	.891
" We.....	.811	1.439 ¹	" Rea.....	.682	.890
			" S.....	.870	.889
" Bart.*.....	.972	1.014	" A.*.....	.717	.719
" L.*.....	.816	.757	" B.*.....	.784	.743
" Mi.*.....	.893	.724	" Ki.*.....	.775	.734
" Milln.*.....	1.060	.802	" Kn.*.....	.561	.528
" Pe.*.....	.888	.761	" Li.*.....	.827	.821
" Wa.*.....	1.136	.975	" Mac.*.....	.736	.698
			" Rei.*.....	.820	.754
			" V.*.....	.862	.912

¹ Some of the individual reaction-times from which this mean was computed were over 2 secs. But, since, as before mentioned (see above p. 117), the maximum range of the chronoscope used was only 2 secs., we reckoned them at only 2 secs. each in computing the mean. The direction of the results, it will be noted, however, were not obscured by this method.

² In the case of Mr. N., two of the individual reaction-times for "meaning" exceeded 2 secs., but were reckoned at only 2 secs. in computing the mean.

³ In the case of Mr. N., all of the individual reaction-times for "visual image" exceeded 2 secs., but were reckoned at 2 secs. in computing the mean.

The second column on each side gives the arithmetical mean time for the subject's reaction to meaning; and the third column the arithmetical mean time for his or her reaction to visual image.

It will be observed that by far the greater majority of the subjects (those whose names are not starred) showed a decidedly shorter reaction time for meaning than for visual image. And this holds in about equal proportions for men

and women. They belong, in short, to the type represented both by Dr. Moore's 8 subjects and by the writer himself. Their introspections also bear out this conclusion. They all agree in reporting a meaning which appeared before visual image.

Turning now, however, to the subjects whose names are starred, we find a group for whom the results are quite different. All but two of them, Mr. Bart. and Miss V., gave mean reaction times which were actually longer for meaning than for visual image, and these two gave introspections of a character which, combined with the closeness of their reaction time, suggested that a longer and more careful examination of them might have resulted in their cases also, in a longer reaction time for meaning than for visual image.

GROUP I

TABLE II

SUBJECT MISS B.

Meaning	T.	V.	Visual Image	T.	V.
Fork.....	1.030	.092	Square.....	1.490	.337
Turkey.....	.885	.053	Ring.....	.692	.461
Banjo.....	.800	.138	Lion.....	1.025	.128
Rose.....	1.345	.407	Candle.....	.598	.555
Fly.....	.872	.066	Steamer.....	.872	.281
Book.....	.814	.124	Circle.....	1.362	.209
Nest.....	.651	.287	Sofa.....	1.370	.217
Boot.....	.822	.116	Tree.....	.915	.238
Heart.....	.945	.007	Stocking.....	1.370	.217
Jug.....	.658	.280	Stairs.....	.906	.247
Oven.....	.782	.156	Crown.....	.990	.163
Knife.....	.752	.186	Tower.....	1.230	.077
Cat.....	.965	.027	Spoon.....	1.505	.352
Cradle.....	.690	.248	Cherry.....	1.350	.197
Rooster.....	1.525	.587	Brush.....	1.006	.147
Mouse.....	.772	.166	Drum.....	.960	.193
Snake.....	1.012	.074	Hammer.....	1.308	.155
Sled.....	1.356	.418	Pear.....	1.140	.013
Mask.....	1.125	.187	Peacock.....	.978	.175
Flag.....	.580	.358	Skull.....	1.708	.555
Letter.....	.772	.166	Flask.....	1.620	.467
Rabbit.....	1.268	.330	Chain.....	.868	.285
Anchor.....	1.300	.362	Tiger.....	1.240	.087
Apple.....	.782	.156			
Total.....	22.503	4.991	Total.....	26.503	5.756
Mean =	.938	.208	Mean =	1.152	.250
Median =	.847		Median =	1.140	

TABLE III

Miss L.

Meaning	T.	V.	Visual Image	T.	V.
Turkey.....	.560	.044	Square.....	.710	.136
Banjo.....	.532	.016	Ring.....	.518	.056
Rose.....	.511	.005	Lion.....	.655	.081
Candle.....	.391	.125	Steamer.....	.605	.031
Fly.....	.633	.117	Circle.....	.723	.149
Nest.....	.330	.186	Book.....	.520	.054
Boot.....	.482	.034	Sofa.....	.660	.086
Heart.....	.404	.112	Tree.....	.589	.015
Jug.....	.478	.038	Stocking.....	.560	.014
Oven.....	.505	.011	Stairs.....	.360	.214
Knife.....	.405	.111	Crown.....	.482	.092
Tower.....	.390	.126	Cat.....	.568	.006
Cradle.....	.590	.074	Spoon.....	.630	.056
Rooster.....	.582	.066	Cherry.....	.620	.046
Hat.....	.605	.089	Scissors.....	.515	.059
Mouse.....	.545	.029	Brush.....	.532	.042
Snake.....	.672	.156	Drum.....	.515	.059
Sled.....	.665	.149	Hammer.....	.705	.131
Peacock.....	.510	.006	Mask.....	.510	.064
Ladder.....	.520	.004	Flag.....	.555	.019
			Skull.....	.512	.062
Total.....	10.310	1.498	Total.....	12.044	1.472
Mean =	.516	.075	Mean =	.574	.070
Median =	.516		Median =	.560	

To make sure of this result, however, we subjected these 14 subjects to further tests, and, in doing so, we decided to copy Mr. Moore's method exactly, rather than to use the preceding 'discrimination between black and white' method.¹

The words presented to the subjects were one and two syllable names of common objects. The subjects were sometimes instructed to react as soon as they obtained the meaning of the word, sometimes as soon as they obtained a visual image of the object which the word named. The instructions 'react to meaning,' and 'react to visual image' were in the case of any one subject distributed irregularly, but with approximately equal frequency. A majority of the same words

¹ The black and white method was adopted originally with the idea that it would set a more definite check, in the case of reaction to meaning, by making sure that the subject really understood and not merely recognized the word. The results, however, were so exactly similar to those obtained from Mr. Moore's method, that it was thought as well to adopt the latter.

were used over again for the successive subjects in such a way that the same word would with one subject call for the instruction 'react to meaning,' and with another that of 'react to visual image.' Introspections were asked for after about half the reactions only, owing to insufficient time, as each subject could spare but a little over an hour, and it was desired to obtain from each as long an objective series as possible. Nonsense words, introduced occasionally, served as checks to make sure that the subject was reacting fairly. The results of the first few reactions in the case of each subject were discarded. The writer served as sole experimenter throughout this series.

Tables II.-XV. inclusive present the results for the different subjects. In the first column are the words on which the subjects received the instruction 'react to meaning'; in the second column are the reaction times for these words; and in the third column are the deviations of these

TABLE IV

MR. MILLN.

Meaning	T.	V.	Visual Image	T.	V.
Turkey.....	.965	.052	Fork.....	.968	.022
Banjo.....	.748	.165	Square.....	1.172	.226
Lion.....	.856	.057	Ring.....	.942	.004
Candle.....	.865	.048	Rose.....	.905	.041
Circle.....	.930	.017	Fly.....	.818	.128
Book.....	.952	.039	Steamer.....	.995	.049
Sofa.....	1.040	.127	Nest.....	.992	.046
Tree.....	.701	.212	Boot.....	.872	.074
Stocking.....	.962	.049	Heart.....	.972	.026
Stairs.....	.950	.037	Jug.....	.965	.019
Crown.....	.917	.004	Oven.....	.905	.041
Knife.....	.924	.011	Cat.....	1.102	.156
Tower.....	.868	.045	Spoon.....	1.005	.059
Cradle.....	1.086	.173	Cherry.....	.818	.128
Snake.....	1.283	.370	Rooster.....	.840	.106
Hammer.....	.790	.123	Scissors.....	1.016	.070
Sled.....	.950	.037	Hat.....	.814	.132
Mask.....	.692	.221	Brush.....	.873	.073
Peacock.....	.940	.027	Mouse.....	.985	.039
Letter.....	.818	.075	Drum.....	.970	.024
Flask.....	.940	.027			
Total.....	19.177	1.916	Total.....	18.929	1.463
Mean =	.913	.091	Mean =	.946	.073
Median =	.930		Median =	.967	

GROUP 2

TABLE V

MR. L.

Meaning	T.	V.	Visual Image	T.	V.
Banjo.....	.698	.021	Ring.....	.785	.030
Lion.....	.680	.039	Fly.....	.640	.115
Rose.....	.750	.031	Circle.....	.810	.055
Candle.....	.660	.059	Book.....	.722	.033
Steamer.....	.760	.041	Nest.....	.925	.170
Sofa.....	.927	.208	Boot.....	.718	.037
Tree.....	.768	.049	Heart.....	1.378	.623
Stocking.....	.773	.054	Ear.....	.775	.020
Jug.....	.720	.001	Stairs.....	.640	.115
Oven.....	.735	.016	Crown.....	.812	.057
Hen.....	.600	.119	Snail.....	.760	.005
Basket.....	.830	.111	Eye.....	.620	.135
Table.....	.713	.006	Bag.....	.653	.102
Star.....	.665	.054	Horse.....	.770	.015
Nose.....	.658	.061	Trunk.....	.620	.135
Spade.....	.748	.029	File.....	.598	.157
Shoe.....	.750	.031	Picture.....	.628	.127
Balloon.....	.615	.104	Scissors.....	.822	.067
Egg.....	.554	.165	Rooster.....	.672	.083
Chair.....	.782	.063			
Total.....	14.386	1.262	Total.....	14.348	2.081
Mean =	.719	.063	Mean =	.755	.110
Median =	.728		Median =	.722	

times from the mean. Similarly, in the fourth, fifth, and sixth columns are the words, times, and deviations for the instruction 'react to visual image.'

Examining these tables we discover, first, a group of 3 subjects, Group 1, Tables II., III., and IV., who appear to belong after all to the type represented by Dr. Moore's 8 subjects. For all 3 both the mean and the median times for reaction to meaning are shorter than for reaction to visual image. Next, we note 3 subjects, Group 2, Tables V., VI., and VII., whose figures begin to point in the opposite direction, in that, whereas their median times are shorter for meaning, their mean times are shorter for visual image. Finally, we note 8 subjects, Group 3, Tables VIII. to XV. inclusive, who substantiate our original hypothesis, in that for them both median and mean times are shorter for visual image than for meaning.

In order to be sure of these results, however, we must compare the introspections. In parenthesis, be it noted that our subjects were untrained,¹ and hence the value of their introspection is subject to qualification. The attempt was made, however, to make their introspective task as definite

TABLE VI

MISS MAC.

Meaning	T.	V.	Visual Image	T.	V.
Square.....	.600	.011	Fork.....	.580	.013
Ring.....	.589	.022	Turkey.....	.550	.043
Rose.....	.535	.076	Banjo.....	.781	.188
Candle.....	.530	.081	Lion.....	.640	.047
Fly.....	.479	.132	Steamer.....	.598	.005
Circle.....	.623	.012	Book.....	.489	.104
Nest.....	.970	.359	Sofa.....	.861	.268
Boot.....	.501	.110	Tree.....	.542	.051
Heart.....	.546	.065	Stairs.....	.542	.051
Stocking.....	.390	.221	Oven.....	.633	.040
Jug.....	.522	.089	Crown.....	.710	.117
Knife.....	.530	.081	Cat.....	.462	.131
Tower.....	.443	.168	Spoon.....	.515	.078
Cradle.....	.778	.167	Cherry.....	.443	.150
Scissors.....	.575	.036	Rooster.....	.590	.003
Brush.....	.760	.149	Hat.....	.442	.151
Mouse.....	.733	.122	Drum.....	.600	.007
Snake.....	.732	.121	Hammer.....	.478	.115
Sled.....	.740	.129	Pear.....	.660	.067
Mask.....	.518	.093	Peacock.....	.598	.005
Flag.....	.580	.031	Ladder.....	.655	.062
Skull.....	.491	.120	Letter.....	.561	.032
Rabbit.....	.713	.102	Flask.....	.648	.055
Anchor.....	.785	.174	Tiger.....	.500	.093
Apple.....	.635	.024	Hand.....	.608	.015
Hen.....	.656	.045	Basket.....	.633	.040
Snail.....	.605	.006	Table.....	.696	.103
Eye.....	.560	.051			
Total.....	17.119	2.797	Total.....	16.015	2.034
Mean =	.611	.100	Mean =	.593	.075
Median =	.585		Median =	.598	

and easy as possible. The method we chose was to ask them such specific questions as: "Did you get a visual image?" (in case of reaction to meaning). "Did you get a meaning?" (in case of reaction to visual image); and "Which came first?" Occasionally, in the case of a subject who seemed to

¹ The great majority of them were but just finishing their first course in psychology. See above, page 116.

show native aptitude for introspection, he was asked to describe, if he could, his consciousness of meaning; *i. e.*, whether it seemed to him reducible to images or something unique and not further analyzable. Descriptions of the visual images obtained were also asked for from time to time. Any other specific questions put to the subjects will be noted along with the answers obtained.

TABLE VII

MISS REI.

Meaning	T.	V.	Visual Image	T.	V.
Fork.....	.808	.061	Turkey.....	.910	.232
Square.....	.908	.161	Banjo.....	1.132	.454
Ring.....	.790	.043	Rose.....	.704	.026
Lion.....	.632	.115	Fly.....	.716	.038
Candle.....	.663	.084	Circle.....	.620	.058
Steamer.....	.680	.067	Nest.....	.762	.084
Book.....	.685	.062	Boot.....	.620	.058
Sofa.....	1.012	.265	Tree.....	.603	.075
Heart.....	.748	.001	Stairs.....	.538	.140
Stocking.....	.678	.069	Oven.....	.695	.017
Jug.....	1.002	.255	Crown.....	.872	.194
Cat.....	.905	.158	Knife.....	.755	.077
Cherry.....	.580	.167	Spoon.....	.772	.094
Rooster.....	.660	.087	Sled.....	.511	.167
Hat.....	.690	.057	Pear.....	.728	.050
Brush.....	.630	.117	Mask.....	.920	.242
Mouse.....	.745	.002	Peacock.....	.722	.044
Drum.....	.900	.153	Flag.....	.601	.077
Letter.....	.698	.049	Ladder.....	.700	.022
Flask.....	.682	.065			
Rabbit.....	.590	.157			
			Total.....	12.881	2.149
Total.....	15.686	2.195	Mean =	.678	.113
Mean =	.747	.105	Median =	.716	
Median =	.690				

Turning to the three subjects, Group 1, whose results agreed with those obtained by Dr. Moore, we find substantial agreement between their introspection and that of his subjects. When instructed to react to meaning, our three subjects reported, almost without exception, that a consciousness of meaning appeared first, and that a visual image did not come until after this meaning. An interesting thing is, however, that with these three subjects the visual image almost always did come finally. Miss B. reported only

3 cases, Miss L. one case and Mr. Milln. no cases in which a visual image did not follow the meaning. This shows what readily visualizers the three subjects were, and how near they were to crossing the border beyond which visual image comes before, or at least simultaneously with, meaning. When instructed to react to visual image, their introspections were

GROUP 3
TABLE VIII
Miss A.

Meaning	T.	V.	Visual Image	T.	V.
Spoon.....	.550	.020	Tower.....	.325	.087
Cradle.....	.538	.032	Cherry.....	.370	.042
Hat.....	.595	.025	Brush.....	.422	.010
Mouse.....	.390	.180	Snake.....	.681	.269
Drum.....	.558	.012	Pear.....	.900	.488
Hammer.....	1.000	.430	Mask.....	.392	.020
Sled.....	.484	.086	Peacock.....	.455	.043
Flag.....	.579	.009	Skull.....	.504	.092
Flask.....	.505	.065	Letter.....	.345	.067
Rabbit.....	.560	.010	Chain.....	.545	.133
Tiger.....	.572	.002	Apple.....	.408	.004
Hen.....	.582	.012	Eye.....	.366	.046
Hand.....	.589	.029	Table.....	.305	.107
Snail.....	.692	.122	Crab.....	.379	.033
Basket.....	.491	.079	Nose.....	.212	.200
Bag.....	.805	.235	Spade.....	.375	.037
Star.....	.670	.100	Pig.....	.245	.167
Horse.....	.417	.153	Balloon.....	.315	.097
Trunk.....	.498	.072	File.....	.278	.134
Shoe.....	.482	.088			
Egg.....	.560	.010			
Chair.....	.417	.153			
Total.....	12.534	1.924	Total.....	7.822	2.076
Mean =	.570	.087	Mean =	.412	.109
Median =	.559		Median =	.375	

practically the same as those just discussed. They reported a consciousness of meaning which came first, and a visual image which came afterwards.

Below we give sample introspections for both kinds of instructions.

INTROSPECTION—GROUP I

*Instruction: Meaning.**Instruction: Visual image.**Subject Miss B.¹**Cat:* "Thought of cat at home and then saw it."*Tower:* "I thought of the meaning before I saw it."*Snake:* "Thought of crawling thing, visual image afterwards."*Spoon:* "I got meaning before image. I think I always do get meaning first."*Subject Miss L.**Banjo:* "An idea of a banjo. After pressing key saw an image."*Steamer:* "Saw the one I have so often seen on the lake. Had a general idea of a steamer first. I think the general idea always comes first."*Rose:* "No image until long afterwards."*Nest:* "An idea of a nest. Then image of a nest in a tree."*Scissors:* "A general idea, then saw all sorts of scissors, then selected one of those, a pair of embroidery scissors."

TABLE IX

MR. MI.

Meaning	T.	V.	Visual Image	T.	V.
Square.....	.662	.087	Fork.....	.460	.049
Banjo.....	.480	.095	Turkey.....	.370	.139
Lion.....	.593	.018	Ring.....	.543	.034
Candle.....	.593	.018	Rose.....	.299	.210
Steamer.....	.438	.137	Fly.....	.410	.099
Book.....	.570	.005	Circle.....	.650	.141
Nest.....	.430	.145	Sofa.....	.310	.199
Tree.....	.610	.035	Boot.....	.415	.094
Stocking.....	.680	.105	Heart.....	.632	.123
Stairs.....	.576	.001	Jug.....	.670	.161
Crown.....	.565	.010	Oven.....	.592	.083
Cat.....	.642	.067	Knife.....	.610	.101
Hat.....	.633	.058	Scissors.....	.562	.053
Mouse.....	.865	.290	Brush.....	.430	.079
Mask.....	.600	.025	Sled.....	.468	.041
Flag.....	.528	.047	Peacock.....	.682	.173
Letter.....	.538	.037	Skull.....	.521	.012
Rabbit.....	.555	.020	Flask.....	.475	.034
Tiger.....	.362	.213	Chain.....	.565	.056
Total.....	10.920	1.413	Total.....	9.664	1.881
Mean =	.575	.074	Mean =	.509	.099
Median =	.576		Median =	.521	

¹ This subject and Mr. Milln. did report a few cases in which they seemed to get the image first, but the majority of their introspections were like these quoted.

TABLE X

MR. PE.

Meaning	T.	V.	Meaning	T.	V.
Basin891	.078	Fork608	.130
Square715	.098	Banjo700	.038
Turkey814	.001	Ring562	.176
Lion910	.097	Candle948	.210
Rose910	.097	Fly620	.118
Steamer688	.125	Circle882	.144
Book700	.113	Nest742	.004
Sofa970	.157	Boot678	.060
Tree728	.085	Heart718	.020
Stocking825	.012	Jug840	.120
Stairs805	.008	Oven704	.034
Crown673	.140	Cat680	.058
Knife690	.123	Spoon769	.031
Tower915	.102	Cherry741	.003
Cradle790	.023	Scissors778	.040
Rooster925	.112	Drum773	.035
Hat988	.175	Snake658	.080
Brush840	.027	Hammer970	.232
Mouse848	.035	Pear829	.091
Sled708	.105	Peacock688	.050
Mask798	.015	Ladder621	.117
Flag836	.023	Letter706	.032
Skull750	.063	Rabbit752	.014
Flask904	.091			
Chain708	.105	Total	16.967	1.837
Total	20.329	2.010	Mean =	.738	.080
Mean =	.813	.080	Median =	.718	
Median =	.814				

TABLE XI

MR. BART.

Meaning	T.	V.	Visual Image	T.	V.
Basin800	.172	Fork603	.004
Square660	.032	Turkey882	.275
Banjo672	.044	Ring522	.085
Lion310	.318	Candle768	.161
Rose792	.164	Steamer625	.018
Fly516	.112	Book530	.077
Circle680	.052	Sofa573	.034
Nest608	.020	Tree445	.162
Boot648	.020	Ear508	.099
Heart610	.018	Jug613	.006
Stocking622	.006	Oven602	.005
Stairs623	.005	Crown615	.008
Total	7.541	.963	Total	7.286	.934
Mean =	.628	.080	Mean =	.607	.078
Median =	.636		Median =	.603	

TABLE XII

Miss Ki.

Meaning	T.	V.	Visual Image	T.	V.
Basin510	.045	Finger.....	.450	.077
Turkey.....	.465	.090	Fork.....	.452	.075
Ring.....	.390	.165	Square.....	.519	.008
Lion.....	.750	.195	Banjo.....	.612	.085
Fly.....	.408	.147	Rose.....	.440	.087
Circle.....	.570	.015	Candle.....	.469	.058
Boot.....	.345	.210	Steamer.....	.695	.168
Tree.....	.594	.039	Book.....	.520	.007
Ear.....	.480	.075	Nest.....	.708	.181
Jug.....	.985	.430	Bear.....	.530	.003
Crown.....	.658	.103	Heart.....	.590	.063
Hen.....	.609	.054	Stocking.....	.496	.031
Hand.....	.730	.175	Stairs.....	.573	.046
Snail.....	.472	.083	Oven.....	.563	.036
Basket.....	.506	.049	Eye.....	.495	.032
Crab.....	.569	.014	Cow.....	.698	.171
Glove.....	.582	.027	Table.....	.482	.045
Star.....	.450	.105	Leaf.....	.482	.045
Horse.....	.482	.073	Bag.....	.250	.277
Trunk.....	.550	.005	Nose.....	.518	.099
Total.....	11.105	2.099	Total.....	10.542	1.504
Mean =	.535	.105	Mean =	.527	.075
Median =	.530		Median =	.519	

Subject Mr. Milln.¹

Hammer: "Thought of hammer as something to drive nails, then saw a hammer, no specific hammer."

Mask: "Thought right away of mask as something to prevent detection, then a visual image of black mask that covered about half the face."

Mouse: "Thought of mouse as disagreeable, then saw a little mouse on the floor."

Hat: "Meaning that hat was to cover head, then saw a felt hat."

We may conclude that these 3 subjects, both from their objective results and their introspections, belong, generally speaking, to the type represented by Mr. Moore's 8 subjects.

Let us turn now to the three subjects, Group 2, whose results did not point definitely in either of the two directions. In the cases of two of them, Mr. L. and Miss Mac., this inconclusiveness of the objective results is supported by their introspections. In the case of the third, Miss Rei., the introspection does not bear out this inconclusiveness of

¹ This subject and Miss B. did report a few cases in which they seemed to get the image first, but the majority of their introspections were like those quoted.

TABLE XIII

Miss KN.

Meaning	T.	V.	Visual Image	T.	V.
Cradle.....	.690	.067	Cat.....	.732	.152
Rooster.....	.588	.035	Tower.....	.690	.110
Scissors.....	.698	.075	Spoon.....	.470	.110
Brush.....	.615	.008	Cherry.....	.490	.090
Mouse.....	.765	.142	Hat.....	.472	.108
Snake.....	.735	.112	Drum.....	.663	.083
Sled.....	.618	.005	Hammer.....	.748	.168
Pear.....	.860	.237	Flag.....	.653	.073
Mask.....	.625	.002	Ladder.....	.568	.012
Peacock.....	.578	.045	Skull.....	.581	.001
Flask.....	.590	.033	Letter.....	.663	.083
Chain.....	.522	.101	Rabbit.....	.650	.070
Tiger.....	.530	.093	Anchor.....	.500	.080
Finger.....	.780	.157	Apple.....	.480	.100
Basin.....	.963	.340	Key.....	.350	.230
Easel.....	.751	.128	Fork.....	.708	.128
Banjo.....	.503	.120	Square.....	.780	.200
Rose.....	.420	.203	Turkey.....	.552	.028
Candle.....	.740	.117	Ring.....	.520	.060
Steamer.....	.693	.070	Lion.....	.528	.052
Book.....	.500	.123	Fly.....	.478	.102
Nest.....	.640	.017	Circle.....	.670	.090
Bear.....	.616	.007	Sofa.....	.670	.090
Heart.....	.580	.043	Boot.....	.571	.009
Stocking.....	.632	.009	Tree.....	.482	.098
Stairs.....	.449	.174	Ear.....	.662	.082
Oven.....	.590	.033	Jug.....	.582	.002
Eye.....	.500	.123	Crown.....	.525	.055
Table.....	.480	.143	Hen.....	.645	.065
Crab.....	.575	.048	Hand.....	.498	.082
Glove.....	.482	.141	Snail.....	.510	.070
			Basket.....	.460	.120
Total.....	19.308	2.951	Total.....	18.551	2.803
Mean =	.623	.095	Mean =	.580	.088
Median =	.615		Median =	.570	

objective results, but suggests rather that the subject belongs with the last group, *i. e.*, those for whom visual image definitely comes before instead of after meaning. We will reserve the introspection of this third subject to consider in that connection, and will examine now the introspections of Mr. L. and Miss Mac. only.

Mr. L.'s introspection is unique in that for him the question seemed to be one not so much of meaning *vs.* visual image as of some sorts of images *vs.* other sorts of images. When instructed to react to meaning, he never obtained

TABLE XIV

Miss V.

Meaning	T.	V.	Visual Image	T.	V.
Finger.....	.750	.129	Key.....	.935	.107
Basin.....	1.142	.263	Fork.....	.702	.126
Square.....	.760	.119	Turkey.....	.813	.015
Ring.....	.846	.033	Banjo.....	.861	.033
Lion.....	.790	.089	Candle.....	1.045	.217
Rose.....	.913	.034	Circle.....	.502	.326
Fly.....	.960	.081	Nest.....	.712	.116
Steamer.....	.930	.051	Sofa.....	.652	.176
Heart.....	.870	.009	Boot.....	.732	.096
Scissors.....	.540	.339	Jug.....	1.045	.217
Book.....	.692	.187	Hand.....	.690	.138
Tower.....	.752	.127	Bag.....	1.045	.217
Crown.....	1.290	.411	Star.....	.852	.024
Horse.....	.938	.059	Nose.....	1.000	.172
Trunk.....	1.005	.126			
Total.....	13.178	2.057	Total.....	11.586	1.980
Mean =	.879	.137	Mean =	.828	
Median =	.870		Median =	.833	

TABLE XV

Mr. Wa.

Meaning	T.	V.	Visual Image	T.	V.
Key.....	1.240	.247	Finger.....	.840	.061
Fork.....	1.200	.207	Basin.....	.860	.081
Square.....	.780	.213	Turkey.....	.584	.195
Ring.....	1.008	.015	Banjo.....	.712	.067
Lion.....	1.148	.155	Steamer.....	.930	.151
Rose.....	.890	.103	Circle.....	.803	.024
Candle.....	.985	.008	Book.....	.652	.127
Fly.....	1.065	.072	Nest.....	.745	.034
Boot.....	1.108	.115	Sofa.....	.808	.029
Tree.....	.918	.075	Heart.....	.800	.021
Bear.....	.899	.094	Stocking.....	.715	.064
Jug.....	.692	.301	Ear.....	.890	.111
Oven.....	.975	.018	Stairs.....	.788	.009
Total.....	12.908	1.623	Total.....	10.127	.974
Mean =	.993	.125	Mean =	.779	.075
Median =	.985		Median =	.800	

anything imageless, but merely images of one kind or another; sometimes visual, but often cutaneous or kinesthetic. When instructed to react to visual images, he obtained visual image. The following are typical introspections:

INTROSPECTION—GROUP 2

Subject Mr. L.

Instruction: Meaning.*Steamer:* "Visual image of white steamer out on lake. Feeling of tension, i. e., a kind of mental muddle before image."*Stocking:* "Kinæsthetic image, followed by visual image. They came almost simultaneously. Meaning not really made clear until the visual image."*Spade:* "Combination of visual kinæsthetic image. Visual came first."*Instruction:* Visual image.*Heart:* "A feeling of tension or strain, then visual image. No consciousness of meaning before the image."*Horse:* "Saw a dappled gray horse."*Turkey:* "A feeling of tension then a visual image."

Occasionally for 'meaning' he did report the presence in consciousness of the 'idea' of the object named. When asked, however, to analyze this 'idea,' he invariably reduced it to images; kinæsthetic, cutaneous, or even visual, but in that case an image of something other than the immediate object itself. The following are typical:

Instruction: Meaning.*Nose:* "Idea of sniffing (seemed kinæsthetic and cutaneous)."*Star:* "Idea of star as a source of light. (This idea a visual image of light.)" "Visual image of a star after reacting."

We conclude that as far as his introspection goes meaning for this subject was always image, of one sort or another. If this be true, it suggests that the reason his objective time for meaning averaged about the same as that for visual image might be due to the fact that the various kinds of images which he got for 'meaning' took on an average about the same time to develop as the purely visual images which he got when reacting to 'visual image.' In addition, however, to suggesting an explanation of his objective reaction times, this introspective testimony to the effect that meaning was always image of one kind or another, would carry, of course, a direct answer to our original problem. It would settle it against the adherents of imageless thought. We cannot, however, be certain of this decision until we are sure of the absolute reliability of this subject's¹ introspection. And for

¹ This particular subject happened to be one of the better-trained (as well as one of the most intelligent) students, since at the date when the test was performed he had just completed a year's course in laboratory psychology, in addition to having had two previous courses in psychology.

that we lack proof. We will not, therefore, attempt to make any final statement, but simply draw attention to this subject's introspection and to the way in which it points.

Turning now to the introspection of Miss Mac., the second subject for whom the objective time appeared approximately the same for both meaning and visual image, we find a somewhat different state of affairs. This subject did not succeed in analyzing her consciousness of meaning. The presumption was, therefore, that it was unanalyzable because imageless. The approximate equality of her times for meaning and visual image, however, is explainable by the fact that, introspectively, this awareness of meaning appeared sometimes before the visual image and sometimes afterwards. Out of 16 times in which she was asked to introspect upon reaction to meaning, she reported 6 times in which meaning came before the visual image, and 10 times in which image came before meaning. When instructed to react to visual image, she reported 8 times in which meaning came before the visual image and 8 times in which the visual image came before the meaning. Below are typical introspections:

INTROSPECTION—GROUP 2

Miss Mac.

Instruction: Meaning.

Snake: "Image first, then knew what it was, knowing a sort of memory of snakes that I have seen."

Gradle: "Knew what it was when I saw it."

Hen: "Visual image first; then recognized the image. I remembered that I had seen things before that looked like that."

Instruction: Visual image.

Drum: "Knew what it was, then saw drum being carried in a parade."

Peacock: "Saw it, then meaning."

Ladder: "Knew what it was, then image."

This subject's introspection, as far as it goes, substantiates her objective results and we see why, in her case, reaction to meaning and reaction to visual image required about equal average times.

Combining her results with those of Mr. L., we note that these two subjects taken together constitute a transition group in that for them both reaction to meaning seems some-

times though not always to depend upon visual image. In Mr. L.'s case meaning sometimes actually *was* visual image; in Miss Mac.'s case it sometimes followed the visual image and was dependent upon the latter.

We may turn now to the introspection of the third and last group of subjects, the group for whom reactions to meaning, as we shall see, did always depend upon a visual image. Miss Rei., it will be remembered, belonged introspectively to this group. In considering the introspection of these 9 subjects (including Miss Rei.) we find it possible to divide them, roughly speaking, into two subgroups: first, a subgroup, *A*, composed of subjects who, as a rule, tend to make a distinction between meaning and visual image; and second, a subgroup, *B*, made up of subjects who, as a rule, do not tend to make such distinction, but for whom meaning *is* visual image.

Subgroup *A* comprises Miss A., Mr. Mi., Mr. Pe., and Miss Rei.

Subgroup *B* comprises Mr. Bart., Miss Ki., Miss Kn., Miss V., and Mr. Wa.

The following are typical introspections for subgroup *A*.

GROUP 3—SUBGROUP *A*

Instruction: Meaning.

Instruction: Visual image.

Subject Miss A.

Rabbit: "Saw a rabbit hopping. Realized it was a small animal, then reacted."

Letter: "I saw a letter lying open. No particular letter."

Drum: "First saw a picture of a man beating drum in orchestra, and then thought, 'it is a musical instrument.'"

File: "Saw both a letter file, and a file, the tool."

Subject Mr. Mi.¹

Tree: "Saw a bunch of trees, then thought what a tree really is."

Scissors: "Image first; didn't think what it meant till after I pressed the key."

Stairs: "Saw stairs, then got a meaning, i. e., stairs something that you go up on."

Skull: "Image and that was all."

¹This subject reported some instances in which he thought he obtained a meaning without a preceding image. His introspection tends, therefore, to class him to some extent in group 2 with Miss Mac rather than here in group 3.

Subject Mr. Pe.

Cradle: "Saw a cradle first, and then the idea to rock came on."

Rooster: "Saw a big black rooster we used to own, and then thought of him as something to eat."

Scissors: "Saw scissors lying on a sewing-table, that belonged to my mother."

Snake: "Visual image of a little green garter snake running through the grass."

Subject Miss Rei.

Cherry: "Saw a red cherry, then thought of it as something to eat."

Drum: "Saw a drum and then thought of it as being a musical instrument."
(In answer to question) "I think the image is an aid to meaning."

Mask: "Saw a black mask, nothing besides the image."

Ladder: "Saw a tall painters' ladder, no consciousness of meaning."

Peacock: "Saw a peacock going up a hill. It was at Lincoln Park."

Examining these introspections, we note that in the case of Subgroup *A* visual image was always a precursor of meaning, and that the meaning itself seemed to depend upon the image. Turning now to Subgroup *B*, the following are typical introspections:

GROUP 3—SUBGROUP *B*

Instruction: Meaning.

Instruction: Visual image.

Subject Mr. Bart.

Lion: "A mixed image of the zoo, i. e., the line of cages in Lincoln Park. No other process in consciousness detectable."

Fly: "Visual image of fly paper with flies stuck on it."

Finger: "Image of a finger with ring upon it (no particular finger)."

Ring: "Image of a key ring (no particular key ring)."

Subject Miss Ki.

Fly: "First saw a black fly, then something flying."

Basket: "Saw image of basket at same time that I got meaning. Meaning to me is image."

Crab: "I saw a single crab and the beach behind him filled in."

Nest: "Image of a nest which I saw this morning and then of other nests of all different kinds."

Bear: "Image of a bear in a cage."

Book: "Image of a red book seen at an angle."

Subject Miss Kn.

Nest: "I said the word to myself. I realized it wasn't a nonsense syllable; then I obtained a visual image of a nest on a tree (not any particular nest); then I reacted."

Hen: "Image of a black and white hen."

Snail: "A greenish brown snail shell. Nothing previous to this image."

Oven: "Got an image first thing. As if looking into an open oven."

Basket: "A market basket; nothing before the image."

Stocking: "An image of a new black stocking the first thing in consciousness. Nothing between image and reacting."

Subject Miss V.

Eye: "Made up my mind beforehand that this time I would get a meaning before image, but got an image of an 'eye' almost before I saw the word."

Candle: "First I saw a candle, no particular candle. Then I saw a particular candle, viz., the one I saw last."

Snail: (N. B. reaction to this word took over two seconds and was discarded but the introspection is significant.) "Could not get an image for a long time. Had no meaning until I got the image. Felt just as if I were looking at a nonsense syllable."

Nest: "Image of the nest which I saw last Sunday."

Bag: "Image of travelling bag, also of paper bag."

Subject Mr. Wa.

Ring: "Visual image of ring on my finger. Nothing previous to this image. Afterwards an image of a circle on the ground."

Heart: "Visual image of a human heart."

(In answer to specific question) "I feel that I go through the same process each time no matter whether meaning or visual image is asked for."

In the case of Subgroup *B* the testimony seems to be unanimous that the visual image *was* the meaning. No other process which might play the part of meaning was ever detected either before or after the image.

The common point in the introspection for both subgroups, it will be noted, is that visual image was the first thing which came. The two subgroups disagree, however, as to the extent to which this visual image was important for, or a part of, the meaning. Subgroup *B* declared that it *was* the whole of the meaning. Subgroup *A* reported merely that it always came before the meaning but that meaning itself was something more. This disagreement, together with the fact that for the vast majority of subjects visual image is entirely unneeded for meaning, sets us a problem.

Three solutions suggest themselves. We may try either an out-and-out imageless position, an out-and-out image

position, or some sort of a compromise. The out-and-out imageless position would have to contend that no matter what were the objective reaction times to the contrary, nor what the apparent introspective evidence, the visual image can never have been really, in any true sense, a part of or even necessary for the meaning. Such a position would have to claim that the objectively shorter reaction times for visual image in the case of both subgroups did not prove that the visual image was necessary to the meaning, but that it was merely an adventitious circumstance. It would have to claim also that the introspective testimony from both subgroups as to the subjective precedence in time of the visual image was no additional proof of the prerequisites of the image. The out-and-out imageless position would, in short, have to deny every one of the evidences afforded by the results of our third group of students. In support, it would have only the results of the non-visual¹ subjects, *i. e.*, of Mr. Moore's 8 subjects and those of our investigation who were like his. But since the method we have used is one which traces the importance of the visual image only, the support of the not extremely visual subjects carries little or no weight. We feel justified in concluding, therefore, that our results render the out-and-out imageless position untenable.

We turn now to the out-and-out 'image' position. Results which directly support it are the introspections of Subgroup *B*, that meaning is image, and image is meaning. For it to be completely supported, however, two further demands would have to be satisfied. The objective reaction times for meaning and visual image in the case of Subgroup *B* would have to be the same. This demand was not fulfilled; the reaction times for meaning averaged longer than for image, which implies that meaning for Subgroup *B* as for Subgroup *A* involved something more than mere image. Second, all cases of 'meaning' reported ought to be analyzable by better introspection into images. This would demand that the 'meaning' of Mr. Moore's 8 subjects and of the

¹ Using 'non-visual' in a merely relative sense.

majority of our own original subjects, *i. e.*, the 'meaning' which came before visual image, must really have been imaginal. It must have been made up, that is, of verbal, organic, kinæsthetic, or other images, which these subjects failed to recognize. It would demand, similarly, that Miss Mac.'s 'meaning,' which came sometimes before and sometimes after the visual image, must have been made up of images. And, finally, it would demand that the 'meaning' of Subgroup *A*, which came after the visual image, and which was often similar to an awareness of definition must likewise have been made up of images. None of these demands would be insurmountable, if we were strongly prejudiced in favor of an out-and-out 'image' position. It would be possible to assume, as has been done before, that better introspection would eventually show images in processes where as yet nothing but imageless awarenesses have been discovered. Such an assumption, however, has at the present stage of psychology little but theoretical preconceptions to support it. So that at present we consider it safer to conclude that our results, while they do not completely contradict an out-and-out 'image' doctrine, do nothing actively to support one.

Finally, we may turn to the consideration of an intermediate doctrine which would both allow an essential importance to the image, and yet admit an imageless component as also necessary. Such a doctrine is directly suggested by the results of Subgroup *A*. They, it will be remembered, obtained first of all the visual image and then a 'meaning.' It appeared that for them the visual image was a prerequisite of the 'meaning,' but that the 'meaning' itself was something different from image. It does not seem improbable that a similar situation may have existed also in the cases of the other groups. With the subjects of the Group *I* who obtained 'meaning' first, this 'meaning' may have come after kinæsthetic or organic images which were not identified. And in the cases of Subgroup *B* the doctrine would explain the longer reaction times obtained for meaning than for image. We would simply have to assume that the 'meaning' which

followed the visual image was not recognized by this subgroup, as it was by Subgroup *A*, as something distinct from the image, but was confused with the visual image itself. Such an assumption does not seem at all a difficult one. Further experiments with subjects of the type of Subgroup *B*, *i. e.*, those who introspectively declare meaning to be identical with visual image but who objectively require a longer reaction time for meaning than for image, ought to throw light upon the matter. We hope to be able in the near future to report the results of such experiments.

We may sum up. The results of the 7¹ subjects for whom both objectively and subjectively visual images came first, render an out-and-out imageless position untenable. But the fact, on the other hand, that for 2 of these 7 (Miss A. and Mr. Pe., Subgroup *A*) meaning was distinct from the visual image, as well as that for the great majority of all subjects 'meaning' appears as something not analyzed into images, gives no direct support to an out-and-out 'image' position. A compromise position, therefore, which assumes that 'meaning' depends upon image but is itself distinct from the latter, is the one most nearly suggested by our results.

In conclusion, let us emphasize that the value of the present investigation has lain not so much in the direction of a positive proof of one or the other theory, as in showing that, *if a large enough sample of subjects be taken*, Dr. Moore's method in no way lends support to the out-and-out imageless position.

¹ This excludes Miss Rei. and Mr. Mi. The former's objective times and the latter's introspection left it doubtful whether they really belonged in this third group or in Group 2. See pp. 128-9, and footnote, p. 133.

EXPERIMENTS ON THE RELATIVE EFFICIENCY OF MEN AND WOMEN IN MEMORY AND REASONING¹

BY ARTHUR I. GATES

The majority of psychologists and educators who have expressed themselves on the subject are of the opinion that women, as a rule, are considerably more efficient than men in memory work and less efficient in applying the facts learned, in self-expression, and in reasoning power. For example one writer says:² "Girls excel in learning and memorization accepting studies on suggestion or authority, but are often quite at sea when set to make tasks or experiments that give individuality and a chance for self-expression, which is one of the best things in boyhood."

Opinions similar to these seem to prevail generally among psychologists, educators, and laymen. Many, moreover, are of the opinion that women, in addition to having quicker and more tenacious memories, are as a rule more diligent and painstaking in their work; the boy may often be satisfied with a fair knowledge of the general principles underlying a lesson, while the girl seeks a more detailed and exact knowledge. If such is the fact it should be taken into account in any attempt to determine the sex-differences in memory, for obviously the differences in the time spent on the work might easily account for the differences in the reproduction of the ideas.

The experiments to be described presently were performed first in 1913 and were repeated in 1914 and 1915, using as subjects a class in elementary psychology consisting of from 158 to 275 students of both sexes of the sophomore, junior, and senior years in the University of California.

¹ From the Psychological Laboratory of the University of California.

² Hall, G. S., "Youth: its Education, Regimen, and Hygiene." New York, 1912, p. 287.

The data were obtained from the answers to three sets of questions. Each set of two questions comprised the regular weekly examination of the class. The first set called for a somewhat detailed reproduction of facts presented in the lectures of the week preceding. The second set called for the application of facts or principles given in the lectures, the purpose being to call into action a mental process as closely as possible identical with that involved in reasoning. All the questions were framed by Professor Stratton, who was in charge of the classes, and who endeavored to make the tests as nearly as possible adequate to the purpose of the experiment. The papers in all cases were graded on a basis of ten, but the averages below, for the sake of clearness, are made on the basis of one hundred. All papers were corrected by the regular 'readers,' who were in no case aware that the results were to be used for experimental purposes. It happened, moreover, that each of the nine sets of papers was graded by a different 'reader.'

The following table shows the results in the case of memory questions.

TABLE I
MEMORY

	1913		1914		1915	
	No. of Individuals	Grade	No. of Individuals	Grade	No. of Individuals	Grade
Women.....	95	77	162	89.2	154	86.4
Men.....	59	73	102	85.0	98	81.0
Diff. in favor of women.....		4.0%		4.2%		5.4%

The women show a slight superiority in memory work, amounting on the average to 4.5 percent. While the percentile difference is rather small, its reliability is indicated by the fact that it appears in all cases, although different questions were given at different times to three entirely different groups of individuals.

Table II. shows the average grades obtained by men and women to questions that involved reasoning.

The evidence indicates a slight superiority of the men in this sort of work. The average difference is approximately

TABLE II
REASON

	1913		1914		1915	
	No. of Individuals	Grade	No. of Individuals	Grade	No. of Individuals	Grade
Women.....	90	77.5	153	83.3	154	88.4
Men.....	58	79.5	103	86.4	99	89.2
Diff. in favor of men.....		2.0%		3.1%		0.8%

2 percent, a difference which is so small as to have but little significance were it not for the fact that it is repeated by the three separate groups.

Table III. shows the results of tests in which the subjects were given free choice between a memory and a reason question. The two questions, constituting the regular weekly examination as before, were presented and the students were permitted to take their choice.

TABLE III
ONE MEMORY AND ONE REASON QUESTION
1913

	Memory Question		Grade	Reason Question		Grade
	No. of Individuals	Percent of Individuals of That Sex		No. of Individuals	Percent of Individuals of That Sex	
Women.....	60	72.3	85	23	27.7	86
Men.....	19	28.8	82	47	71.2	87
Diff.....		43.5	3		43.5	1

1914

Women.....	129	84.8	70.4	23	15.2	77
Men.....	80	78.4	64.5	22	21.6	80
Diff.....		6.4	5.9		6.4	3

1915

Women.....	144	91.8	88.4	13	8.2	87.2
Men.....	85	74.2	87.0	16	15.8	89.8
Diff.....		17.6	1.4		7.6	2.6

Although both sexes show a distinct preference for the memory question, the preference is much greater in the case of women. The men show more willingness than do the women to take the reason questions, although the actual number of either sex that take these questions is small. In

1913 and 1915 twice as great a ratio of men, and in 1914 a ratio one third greater of men than of women chose the reason question. The grades received in the memory tests confirm the earlier finding that the women excel in this kind of work. The women excel in every case, although in two (1913 and 1915) the differences in their favor are very small. The grades received on the reason questions also confirm the earlier finding that the men excel slightly in this type of work. Although the superiority of the men is small it appears in every case.

Our general conclusions from the experiment thus far are as follows:

1. The women excel the men in memory work.
2. The men excel the women, but to a less degree, in reason work.
3. Both sexes prefer memory work but more men show a willingness to do reason work in lieu of memory work.

To let the experiment remain as it stands and to accept without further question the conclusions just enumerated would be hazardous and would certainly not take into account all of the factors which have an influence here. There is at least one possibility which if proven to be a reality would force us to modify the conclusions at which we have just arrived. It is possible that the apparent superiority of the women in reproduction from memory is due merely to a greater amount of study and not to an innate superiority of memory.

To take into account this possibility the following test was employed. The news item given below was read to the class at the beginning of the lecture hour, the students being warned to pay particular attention to the contents, without being informed, however, of the purpose of the test. The item follows:

THREE HOUSES BURNED¹

Boston, September 5. A serious fire last night destroyed three houses in the center of the city. Seventeen families are without a home. The loss exceeds fifty thousand dollars. In rescuing a child, one of the firemen was badly burned about the hands and arms.

¹ See Whipple, G. M., 'Manual of Physical and Mental Tests,' Baltimore, 1910, p. 504.

The students were first requested to write down all the facts they could recall from the article. Following the free account, they were asked to answer the following questions:

1. In what city did the fire occur?
2. What was the date of the item?
3. When did the fire break out?
4. How many houses were destroyed?
5. In what part of the city were these houses?
6. How many families were left homeless?
7. What was the total loss (in dollars)?
8. Who was burned?
9. On what part or parts of the body was this individual burned?
10. What was this individual doing when the burns were received?

The data used were obtained from the answers to the ten definite questions, for it was found that the additions or alterations of these answers from the free accounts were so slight as to be negligible. The papers were graded on a basis of ten, one unit being allowed for the correct answer to each question. Table IV. gives the average results.

TABLE IV

	Percentage Reported	Percentage Correct	Percentage Positive Errors	Ratio of Pos. Errors to Amt. Reported	Ratio of Pos. Errors to Pos. Errors Plus Amt. Not Reported
1913					
Women.....	97.4	84.4	13.0	.133	.833
Men.....	90.0	80.0	10.0	.100	.500
1914					
Women.....	98.4	86.4	12.0	.121	.888
Men.....	94.2	83.1	11.0	.116	.650
1915					
Women.....	94.1	82.5	11.6	.123	.662
Men.....	88.0	76.4	10.6	.120	.443
Average of above.					
Women.....	96.6	84.4	12.2	.125	.782
Men.....	90.7	79.8	10.5	.112	.519
Diff.....	5.9	4.6	1.7	.013	.263

The women in every case report a greater amount of the content of the item, as well as a greater amount of it correctly. On the average the women report 96.6 percent of the

item and 84.4 percent of it correctly while the men report but 90.7 percent with 79.8 percent correct. The men, however, make fewer mistakes. The actual number of errors made by the women is greater in every case, although the differences between the sexes is small. The ratios of the number of errors to the total amount reported show even smaller differences because of the fact that the women in all cases report a larger amount. But the ratios of the amount of positive errors to the total amount of positive errors plus the amount not reported—*i. e.*, to the field in which suggestion and kindred forces could operate because the ideas were not correctly remembered—were much larger for the women. That is to say, the women, much more than the men, were likely to make erroneous statements rather than mere omissions. This ratio is, on the average, about one third larger for the women.

The general conclusion from this test is that the women in immediate memory tests can correctly reproduce more of the detail of a given group of facts but at the same time make more mistakes.

A question, however, may be raised with regard to the application of the results gained by this method to the determination of the relative ability shown by men and women in the tests first considered, because the present method tests immediate memory, or immediate reproduction, rather than delayed memory which is the function operative in the examinations.

Accordingly, the same students were requested, one week or five weeks after the immediate-memory test, to write, without previous warning, all that they could remember of the news item given above. The same set of ten questions as before was used. Table V. gives the results.

In delayed as well as in immediate memory the women have a greater range of report, a greater number of details are reported correctly, and more positive errors are made. The amount by which the sexes differ is about the same in both types of memory.

The experiments with the news item justify the following conclusions:

TABLE V
 DELAYED MEMORY
 1913. After 5 Weeks

	Percentage Reported	Percentage Correct	Percentage Positive Errors	Ratio of Posi- tive Errors to Amount Reported	Ratio of Positive Errors to Posi- tive Errors Plus Amount Not Reported
Women.....	82.0	64.0	18.0	.219	.500
Men.....	72.0	57.0	15.0	.208	.349
1914. After 5 Weeks					
Women.....	89.0	68.0	21.0	.236	.655
Men.....	79.0	60.0	19.0	.240	.475
1915. After 1 Week					
Women.....	94.4	80.7	13.7	.145	.650
Men.....	89.8	75.8	12.0	.137	.495
Average of Above					
Women.....	88.5	70.9	17.9	.200	.602
Men.....	80.3	64.3	15.3	.195	.440
Diff.....	8.2	6.6	2.6	.005	.162

1. The women excel the men in tests of immediate or delayed memory, at least in so far as the amount of material correctly reproduced is concerned.

2. The women, however, make more positive errors in reporting.

The results obtained by other investigators are for the most part in harmony with the present findings. A summary of such experiments will be found in Whipple¹ who concludes: "Sex differences in this test [memory for ideas], as in the rote memory test, are in favor of girls."²

A final consideration is the possibility that the women employed in these experiments constitute a more select group than the men. It is possible that these women are on the whole more capable, or that their previous training has better adapted them to the particular subject of psychology. There is no obvious reason why this should be the case, but in order to throw some light upon it the average grade in the course has been computed for each sex.

¹ Whipple, G. M., 'Manual of Mental and Physical Tests,' Part II., 17-43, 149-223.

² *Op. cit.*, p. 213.

TABLE VI

	1913	1914	1915
The women received an average grade of.....	77.0	75.5	74.0
The men received an average grade of.....	75.5	75.0	72.0

The women have slightly the higher grade. The mass of experimental evidence from other investigations, however, indicates that in groups of men and women of equal endowment and training, the women usually excel in memory work. We have found that in the three groups just considered, the women excel in memory. It seems that the small amount by which the women excel the men in the grades received in the course may be accounted for by the great predominance of memory work in the weekly and final examinations on which the grades are based. The women who apparently excel in memory work should in a long series of tests of that nature, come out with a somewhat better average.

The three main conclusions that the investigation seems to justify are as follows:

1. The women excel the men noticeably in either immediate or delayed memory work.
2. The men excel the women, but to a less degree, in reason work.
3. Both sexes prefer memory work, but a greater relative number of men show a willingness to do reason work in lieu of memory work.

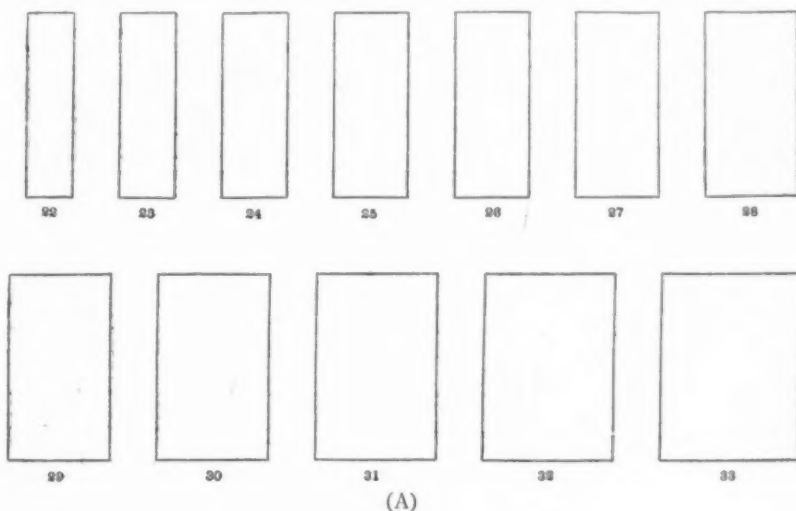
Results obtained by other investigators and the supplementary tests for possible sources of error have brought forth no evidence contradictory to the conclusions we have reached.

INDIVIDUAL DIFFERENCES IN JUDGMENTS OF THE BEAUTY OF SIMPLE FORMS

BY EDWARD L. THORNDIKE

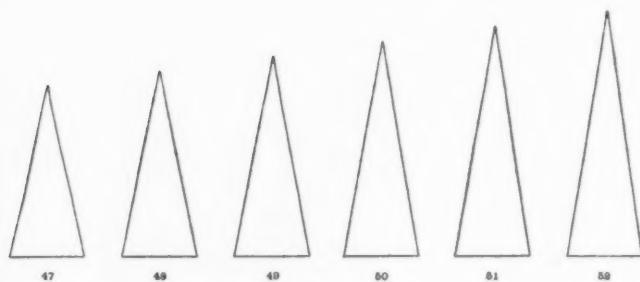
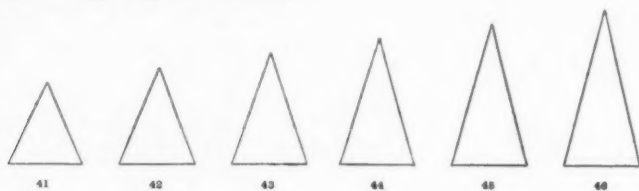
Teachers College, Columbia University

Students of esthetic appreciation have commonly been especially interested in the general drift or average tendency toward this or that preference and have perhaps given an impression of greater uniformity than exists. The diversity of the judgments whose average favors the golden section, for example, is really very great. It seems worth while therefore to report certain rather extensive measurements of esthetic preference which I have made.

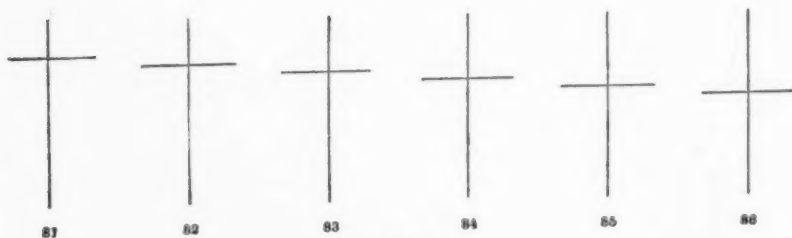
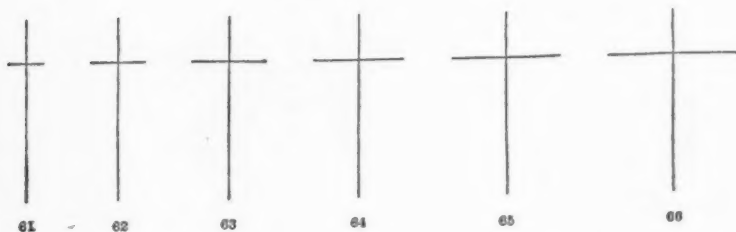


The subjects of the experiment were college juniors and, with few exceptions, of the female sex. The judgments made were of the order of esthetic merit (the question being, "Which rectangle do you like the looks of most? Next most? etc.") of (A) rectangles 22-33, (B) triangles 41-52, (C) Crosses 61-66 and 81-86, (D) designs A-L and (E)

the 24 unnumbered designs. Each set was shown as here save that the dimensions were in each case double those here (quadrupling the areas).



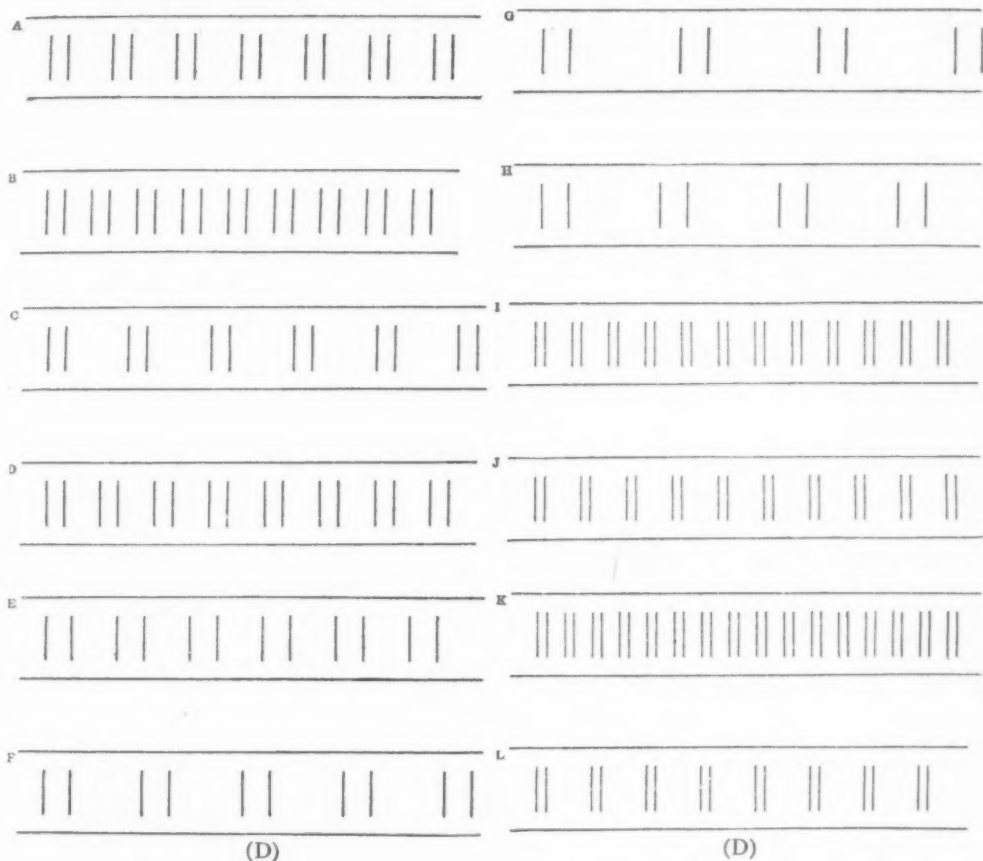
(B)



(C)

I give the facts for from 100 to 250 individuals who made the judgments, in the form of the percent of them assigning a given form to a given position.

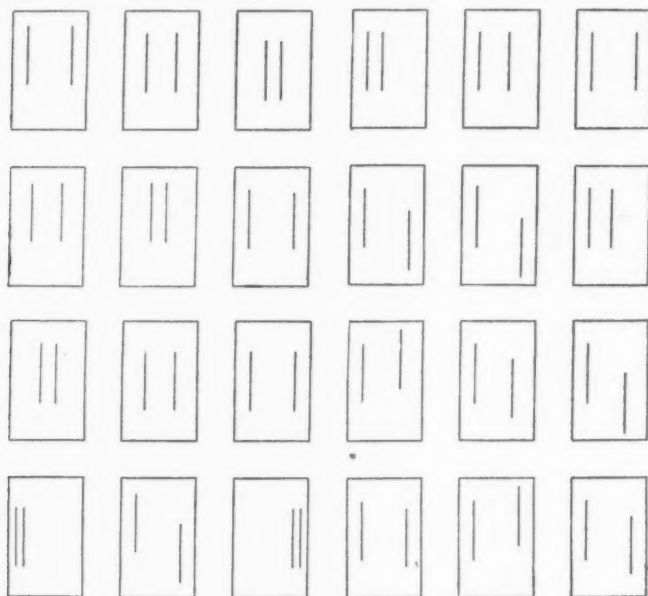
In the case of the rectangles it will be observed that 27, 28, and 29, those most liked, still have some ratings in the lowest position of all; and that 33, the one least liked, still has ratings in the highest position. In only 3 cases out of 144 do over 25 percent of the ratings give a rectangle the same position.



In the case of the triangles there is a pronounced drift of opinion against the tall triangles, but even so almost every position has votes in the case of each. This is still more the case with the crosses.

In the case of the designs where the sequence by proportions is more hidden, the variability becomes enormous.

Although any one person may feel very decided preferences, these are never shared by enough of his fellows to make



(E)

TABLE I

FREQUENCIES OF EACH POSITION FOR EACH RECTANGLE COMPUTED FROM ORDERS OF MERIT REPORTED BY 200 INDIVIDUALS: IN PERCENTS

Positions	Rectangles											
	22	23	24	25	26	27	28	29	30	31	32	33
1	4.5	5	3.5	8.5	6.5	15	10.5	15	16	6.5	4.5	4.5
2	1.5	7	6	7.5	14	7	16	17	11	8	3.5	1.5
3	4	2.5	8	11	9.5	15	15	11	8.5	7	5.5	3
4	2.5	3.5	11	12	10	16.5	13.5	7.5	11	7	4.5	2
5	2	4	5.5	12	18	15.5	8.5	12	8	7	4	3
6	2	4.5	9	14.5	22	9	12.5	10	7.5	5.5	3	1.5
7	5.5	7.5	12	15	4	14.5	9.5	6.5	9	11	4.5	1.5
8	6	9	15.5	6	12.5	2.5	5.5	11	8	11.5	10	1.5
9	9.5	15	12	10	1.5	2	2	5	15	11.5	11	5.5
10	11.5	18.5	14	1.5	1.5	2.5	1	1.5	4.5	22	14	7
11	15	21	3	2	3	2	1.5	3	35	14.5
12	36	2.5	0.5	0.5	0.5	3	1.5	0.5	54.5

anything like universal agreement. In the series of 12 designs, not one has 25 percent of ratings in any one position.

In the series of 24 designs, in only about one case out of thirty are there 10 percent or more of ratings in any one position.

TABLE II

FREQUENCIES OF EACH POSITION FOR EACH TRIANGLE COMPUTED FROM ORDERS OF MERIT REPORTED BY 250 INDIVIDUALS: IN PERCENTS

Positions	Triangles											
	41	42	43	44	45	46	47	48	49	50	51	52
1	26.0	14.0	21.2	14.0	7.2	4.8	3.6	2.0	1.2	3.2	.8	1.6
2	10.8	31.2	16.8	12.0	7.6	6.8	7.2	2.8	2.8	1.6	.8
3	8.8	14.8	30.4	16.4	12.0	3.6	3.6	3.6	3.2	2.0	1.2	.8
4	9.6	7.2	6.8	33.2	14.0	11.2	8.0	4.0	1.6	3.6	.8	.4
5	6.4	5.6	7.2	7.2	36.0	12.0	9.2	8.0	4.0	2.4	1.6	.8
6	7.2	3.2	4.4	6.4	6.4	41.2	12.0	7.6	7.2	.4	2.4	1.6
7	4.4	5.2	3.2	2.4	4.8	6.8	45.6	12.4	6.8	2.8	2.4	2.8
8	2.4	4.8	1.2	1.2	6.8	4.8	4.8	49.2	12.0	8.0	2.4	1.6
9	5.6	4.8	2.4	4.8	2.4	2.8	1.6	5.6	52.0	10.0	4.0	3.6
10	5.2	2.0	4.4	1.2	1.2	3.6	2.0	2.0	4.8	61.2	9.2	4.0
11	6.8	4.8	1.2	.4	.8	2.0	1.6	.8	3.2	4.8	67.6	4.4
12	5.6	2.8	0.8	.4	.84	.8	1.2	1.6	5.6	76.8
?4	1.24	.8

No great value attaches to the general drift of the consensus, since the responses to the objects displayed as they were and with criteria of symmetry so strongly suggested

TABLE III

FREQUENCIES OF EACH POSITION FOR EACH CROSS COMPUTED FROM ORDERS OF MERIT REPORTED BY 140 INDIVIDUALS: IN PERCENTS

Positions	Crosses											
	61	62	63	64	65	66	81	82	83	84	85	86
1	2.1	.7	10.0	15.0	24.3	7.1	6.4	19.3	5.7	2.1	2.1	5.0
2	2.1	3.6	10.0	22.1	16.4	4.3	5.0	14.3	12.9	1.4	7.1	1.4
3	6.4	8.6	15.7	10.0	11.4	4.3	18.6	8.6	10.7	1.4	4.3
4	3.6	3.6	10.0	15.0	9.3	5.0	15.0	13.6	15.0	2.1	6.4	1.4
5	7.9	15.7	7.1	10.7	6.4	13.6	15.0	15.0	4.3	3.6	1.4
6	3.6	8.6	11.4	9.3	5.7	8.6	13.6	7.9	12.1	11.4	3.6	4.3
7	1.4	5.0	11.4	3.6	11.4	12.9	12.1	4.3	15.7	10.7	5.7	5.0
8	5.0	10.0	3.6	2.1	6.4	17.9	10.7	3.6	2.9	24.3	9.3	5.7
9	2.1	11.4	5.0	6.4	2.9	9.3	7.1	2.1	6.4	17.1	24.3	4.3
10	11.4	10.0	13.6	2.9	5.0	5.7	.7	4.3	8.6	17.1	20.7
11	15.0	32.17	2.1	5.7	5.0	.7	1.4	3.6	17.1	15.7
12	53.6	0.7	.77	6.4	1.4	3.6	2.1	30.7

may be different from the responses to the same objects in isolation or in different surroundings. However, it may be of interest to some to record that: The most liked rectangles

TABLE IV

FREQUENCIES OF EACH POSITION FOR EACH LETTERED DESIGN COMPUTED FROM
ORDERS OF MERIT REPORTED BY 100 INDIVIDUALS: IN PERCENTS

Positions	Designs											
	A	B	C	D	E	F	G	H	I	J	K	L
1	14	6	12	9	2	5	6	2	15	13	7	10
2	12	8	12	8	2	2	1	9	13	16	5	11
3	16	2	12	10	10	5	3	5	12	14	4	9
4	16	11	11	11	9	10	3	2	5	9	5	9
5	14	9	12	11	10	7	3	5	7	6	4	12
6	8	6	13	15	6	17	5	6	5	9	1	10
7	11	13	7	6	7	10	11	7	6	6	7	8
8	5	9	11	13	8	8	7	10	4	11	6	7
9	1	6	6	7	19	12	13	6	7	10	12
10	2	7	2	5	12	16	7	19	10	11	3	5
11	1	10	5	11	5	17	22	15	3	7	5
12	13	2	4	3	24	7	1	2	41	2

TABLE V

FREQUENCIES OF EACH POSITION FOR EACH UNNUMBERED DESIGN OF THE FIRST
TWO ROWS COMPUTED FROM ORDERS OF MERIT REPORTED BY 250
INDIVIDUALS: IN PERCENTS. THE RESULTS FOR THE OTHER
TWO ROWS SHOW THE SAME VARIABILITY

Positions	Designs											
	1	1	1	1	1	1	2	2	2	2	2	2
Number	1	2	3	4	5	6	1	2	3	4	5	6
1	2.8	10.0	12.8	2.8	1.2	4.4	2.0	.8	.4	7.2	2.4	1.2
2	4.0	9.6	8.8	2.0	.8	5.2	6.4	4.4	3.2	4.0	4.4	1.2
3	3.6	9.2	8.8	3.6	6.8	4.4	2.8	2.8	4.8	2.8	.4
4	5.6	10.2	7.2	2.4	1.6	3.6	4.4	3.2	3.6	3.6	4.0	3.6
5	6.0	8.0	4.0	2.4	.4	8.0	3.6	6.8	5.6	3.2	2.0	1.6
6	4.8	8.4	3.6	2.4	3.6	8.4	9.2	7.2	7.2	2.8	1.6	2.4
7	8.0	4.4	7.2	3.6	4.8	4.4	7.6	4.8	4.8	4.8	2.8	3.6
8	7.2	3.2	2.0	2.8	4.4	8.4	7.2	6.0	6.0	4.4	4.8	2.8
9	6.0	4.4	1.6	3.6	4.8	7.6	4.8	4.0	4.8	6.4	6.0	3.2
10	3.2	3.6	2.8	4.0	6.0	5.2	4.8	6.8	10.0	4.0	5.6	3.6
11	5.2	2.4	2.8	4.8	5.2	4.8	4.8	4.4	7.2	6.0	4.0	4.4
12	3.2	4.4	4.0	2.8	6.0	3.6	5.2	4.4	6.4	7.2	6.4	5.2
13	5.2	4.8	2.8	6.4	4.0	4.4	4.8	3.6	2.8	3.2	6.0	6.0
14	4.8	3.6	3.2	2.8	4.0	3.2	5.2	2.8	3.6	8.8	5.6	6.4
15	4.0	1.6	2.8	3.2	4.0	3.6	3.6	5.2	4.0	4.8	10.0	4.4
16	4.4	1.6	1.2	4.0	4.8	2.4	6.4	4.8	4.0	4.8	6.0	4.4
17	2.8	2.0	3.2	2.4	4.8	1.6	4.4	3.2	5.6	6.0	3.6	2.4
18	2.0	1.6	3.2	5.6	6.8	1.6	2.0	6.0	2.8	1.2	5.2	4.4
19	3.2	2.0	4.0	4.8	8.4	4.0	1.2	3.2	3.2	4.0	3.6	4.0
20	2.0	1.6	3.2	8.4	5.6	1.6	1.6	4.4	3.2	1.6	4.0	8.4
21	2.8	1.2	3.6	10.8	6.8	2.0	2.8	5.2	2.0	2.4	2.4	10.0
22	3.6	1.2	11.2	3.2	1.6	2.0	4.4	.8	1.2	3.6	10.0
23	1.6	.8	3.2	1.6	3.6	2.4	.8	1.2	1.2	.8	2.4	2.4
24	4.0	.8	1.2	1.2	4.4	.8	.8	.4	4.4	.4	.4	4.0

TABLE VI

ORDER OF MERIT ASSIGNED BY THE CONSENSUS OF COLLEGE STUDENTS

Rectangles	Triangles	Crosses	Lettered Designs	Unnumbered Designs. The Numbers Here Follow the Order of Printing*
29	43, 44	64, 82, 65	A	14
28	42	81, 83, 66	J	2
27 and 30	41, 45	84, 63	C D I L	3
26	46	62, 85	B F	6, 15
25 and 31	47	61, 86	E	1, 7, 13
24	48		G. H.	8, 9, 10, 20
23 and 32	49		K.	11, 17, 22, 24
22 and 33	50			4, 5, 12, 18
	51			16, 23
	52			19, 21

* That is, the first design in the second row is 7, the next is 8; the first design in the third row is 13, the next is 14, etc.

had, as the ratio of altitude to base, 1.83 to 1. The most liked triangles had, as similar ratios, 1.6 to 1 and 1.7 to 1 (43 and 44 being equally well liked). The most liked of the crosses had a bar half of the length of the upright and such a bar is best liked when it cuts the upright so as to leave one fourth above and three fourths below. A bar two fifths of the length of the upright is nearly as well liked. The most liked of the unnumbered designs is the second one of the third row. The first and third of the fourth row are the most disliked. In the lettered designs the space relations may vary widely so long as the design remains obvious, and so long as neither bareness nor crowdedness is suggested. *A* and *J* are liked about equally; *G*, *H* and *K* are disliked about equally.

The order of merit of the consensus is given for each group of designs in Table VI.

PRELIMINARY REPORT ON THE RELATIVE INTENSITY OF SUCCESSIVE, SIMULTANEOUS, ASCENDING, AND DESCENDING TONES

BY A. P. WEISS

Ohio State University

The attribute of tone intensity has been relatively neglected in experiments in audition because of the technical difficulty in producing pure tones which may be varied in their loudness or intensity in a definite and measurable manner.

The apparatus with which the experiments in this paper were performed was developed at the University of Missouri under the guidance and suggestions of Dr. M. F. Meyer. The extended report showing the details of the apparatus construction and the manner in which the experiments were conducted is being published as a *PSYCHOLOGICAL REVIEW MONOGRAPH*.

The apparatus makes it possible to produce tuning-fork tones which meet the following conditions:

1. The tones are pure in the sense that no lower or upper harmonics can be detected.
2. The tones can be quickly varied from weak to strong in any number of steps and each degree of intensity can be repeated as often as necessary.
3. The tones 'come in' and 'go out' at their full intensity without disturbing, starting, or stopping noises.
4. The phase relations of the tuning forks is under control.

The nature of the experiments may be understood from the following illustrations.

1. Relative intensity of successive and simultaneous tones: Suppose we have the tone 200 which, during a given trial, is always sounded at a medium and constant intensity. Another tone 250 can be easily varied from weak to strong

(ascending order)¹ or from strong to weak (descending order). Suppose we sound 200 and 250 *alternately* and vary the intensity of 250 (either ascending or descending) until it seems to have the same intensity as 200; the question now arises, if 200 and 250 are sounded *simultaneously*, are they still of the same intensity?

2. Relative intensity of ascending and descending tones: Suppose 200 is kept constant in intensity and 250 is varied in *descending* order, will the point at which 250 is considered equal to 200 be the same as when 250 is varied in *ascending* order?

The tones used in this experiment were the four tones 150, 200, 250, 300. Each tone was compared with each of the other three tones in four ways.

1. Successively, with the comparison tone varying in ascending order.

2. Successively, with the comparison tone varying in descending order.

3. Simultaneously, with the comparison tone varying in ascending order.

4. Simultaneously, with the comparison tone varying in descending order.

Both the lower and the higher tones were used as standard in each pair. Each pair of tones was further compared for ten degrees of intensity ranging from a weak tone which was nevertheless clearly heard, to a strong tone which was not, however, so loud that it became disagreeable. That is, the range of conveniently obtainable intensities was divided into ten steps and the various tone combinations were compared for each of these steps.

The method of making the judgments was that of "Selbst-einstellung." One tone (the standard) was kept at constant intensity while the observer varied the intensity of the comparison tone until it seemed equal in intensity to the tone which was being used as the standard.

¹The numbers 200 and 250 refer to the vibration rates. Ascending order or ascending tones refer to tones which are varied from silence to weak to strong. Descending order or descending tones refer to tones whose intensity is varied from strong to weak.

The tones were produced by resonators suspended over silently vibrating tuning forks of constant amplitude and the objective intensity of the tone was measured by the distance of the mouth of the resonator from the prongs of the tuning fork.

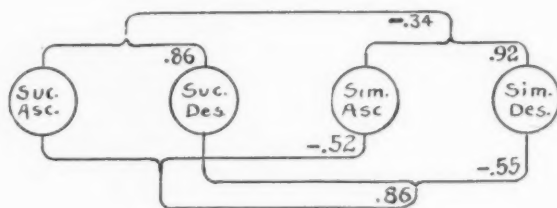


FIG. 1.

The above diagram shows the results obtained. The number in the right arm of each brace indicates the intensity relations between the series connected by the brace. Thus the number $.86$ in the brace connecting the circles of the successive-ascending and successive-descending series, means that when a tone of constant intensity was compared *successively*, first with an ascending tone, and second with a descending tone, the descending tone was made $.86$ step stronger than the ascending tone. Subjectively this implies that the descending tone was actually heard weaker than the ascending tone, since if it had been set at the intensity of the ascending tone it would have been judged weaker than the standard tone.

A negative sign in the diagram means that the series was made weaker (objectively) than the companion series. In subjective terms this means that the tone was actually heard stronger. This opposition between "made stronger objectively" and "heard weaker subjectively" is rather confusing and it was thought worth while to add the subjective implications parenthetically in the statement of the conclusion.

Each of the conclusions which follow are based on at least 4,800 judgments or reactions. The term 'step' refers to one tenth the total range of intensities used in the experiments, or one tenth of the range of conveniently obtainable intensities.

1. When compared *successively* with a tone of constant intensity, *descending* tones are made .86 step stronger (or heard .86 step weaker) than *ascending* tones.

2. When compared *simultaneously* with a tone of constant intensity, *descending* tones are made .92 step stronger (or heard .92 step weaker) than *ascending* tones.

3. When compared with a tone of constant intensity, *ascending* tones, when compared *simultaneously*, are made .52 step weaker (or heard .52 step stronger) than when compared *successively*.

4. When compared with a tone of constant intensity, *descending* tones, when compared *simultaneously*, are made .55 step weaker (or heard .55 step stronger) than when compared *successively*.

5. When compared with a tone of constant intensity in mixed simultaneous and successive order, *descending* tones are made .86 step stronger (or heard .86 step weaker) than *ascending* tones.

6. When compared with a tone of constant intensity in mixed ascending and descending order, *simultaneous* tones are made .34 step weaker (or heard .34 step stronger) than *successive* tones.

The deviations of one half of the intensity judgments above or below the objective intensity was .74 step. This seems to indicate that within the range of conveniently obtainable intensities used in this experiment 15 steps might have been discriminated. Taking very weak and very loud tones it seems that 25 steps should be possible.

The value .74 is also an indication of the reliability with which intensity judgments can be made. This is about the same for all the intensities used, being somewhat less for the medium intensities than for the extremes, as might have been expected.

The variability of the intensity judgments is not influenced as much by difference in vibration rates as was expected. The greatest difference between any of the tones of this experiment was an octave (150 vibrations) and the comparisons

between these two tones were no more variable than where the difference was 50 vibrations. This seems to show that even between tones whose vibration difference is considerable, the intensity judgments can be made with a degree of accuracy which promises well for an experimental analysis of the sound intensity reaction.

DISCUSSION

A NEW METHOD OF HETEROCHROMATIC PHOTOMETRY—A REPLY TO DR. JOHNSON

In the September number of this journal appears a discussion entitled 'A Note on Ferree and Rand's Method of Photometry,' by Dr. H. M. Johnson, of the Nela Park Laboratory. This discussion, we may perhaps be pardoned for noting, is remarkable chiefly for its numerous mistakes and incorrect or misleading representations, a few of which we take opportunity here to rectify. The net service of the discussion is to call the authors' attention to the omission of a decimal point in the original article, for which they duly acknowledge their debt.

1. In his opening paragraph Dr. Johnson says: "The authors claim for their method that with respect both to sensitivity and reproducibility it surpasses the equality of brightness method, even when the photometer head used is of the best Lummer-Brodhun type." In regard to this statement we beg to point out that Dr. Johnson has omitted from what was actually said all that makes a difference between a reasonable and an absurd claim. We had claimed in our paper greater reproducibility of setting for the method in question as compared with the equality of brightness method *only in case of heterochromatic photometry*, in which respect as is well known the equality of brightness method is notably deficient. The possibility of a service to heterochromatic photometry alone is the reason given in the paper for applying to the rating of artificial lights a principle formerly used by us for an entirely different purpose. Also the special reference to heterochromatic photometry was featured in the title.

2. Dr. Johnson next says: "The authors assumed that the two elements making up the photometer screen 'received equal amounts of light from the source to be measured.' Even if the elements were equidistant from the lamp . . . the truth of this assumption does not follow from the data given. In some of the work the results of which are presented in the authors' table, the angular separation of the compared elements was 14° to 15° at the source. Now the radiation from a carbon or tungsten lamp is not equal in

all directions as is that from an ideal point source. In fact, for lamps of such types, differences of several per cent. in different directions normal to the long axis of the lamp are the rule, and a considerable difference might occur in a range of 15° ."

With reference to the above statements we wish to note in the first place that it was never assumed by us that there were only two elements in the photometer screen. This erroneous interpretation of the principle on which the method is based can be attributed to Dr. Johnson only. Secondly, that when the angular separation of the elements referred to (the stimulus patch and the measuring disc), is correctly computed from the data contained in the original article it is found to vary between 4.5° and 11° ,¹ and not to have a range of 15° . And thirdly, that when the question of the influence of the distribution curve on the general applicability of the method to working practice was raised by us in a paper presented to the Philadelphia Section of the Illuminating Engineering Society in February, 1914,² it was the consensus of opinion in the discussion that followed that the possibility of error from this source is of negligible consequence in a field presenting so many difficulties as heterochromatic photometry, and that the effective check on these and many other points which were raised by us at that time—in addition to those now raised by Dr. Johnson—must come in a comparison of the results with those obtained by the equality of brightness method. Because of this confirmatory opinion of a group of specialists fully familiar with all the technical and working details of photometry and because of the check experiments we had run on the point to convince ourselves of the negligible influence of the factor for the conditions under which we worked (see this paper, p. 165), we had not considered it necessary to raise the discussion in the preliminary exposition of the principles on which the proposed method is based, contained in the article in question. However, since the point has been raised by Dr. Johnson, the following comments may not be out of place.

¹ It is assumed here of course that Dr. Johnson referred to the angle for the colorless light. There can have been no reasonable doubt in his mind that the colored light was not obtained from the naked carbon or tungsten lamps to which his comments on distribution refer. (See footnote, original article p. 9).

² With reference to the foregoing point and to others taken up in this discussion it is scarcely needful to state that principles and descriptions of conditions of a technical nature were taken up in a fuller and more detailed way when a statement of the method was presented to auditors technically interested in photometry than was done in the article criticized by Dr. Johnson.

(a) A general statement of the type which Dr. Johnson has made about the inequality of distribution of carbon and tungsten lamps is incomplete to the point of being somewhat misleading. As is well known, the distribution curve of an incandescent filament lamp depends upon the shape of the filament. While, for example, the single oval filament of the ordinary carbon lamp gives considerable unevenness of distribution, if wide enough angles are considered, the single loop tungsten filament of the Mazda lamp, series type, gives a curve which deviates so little from

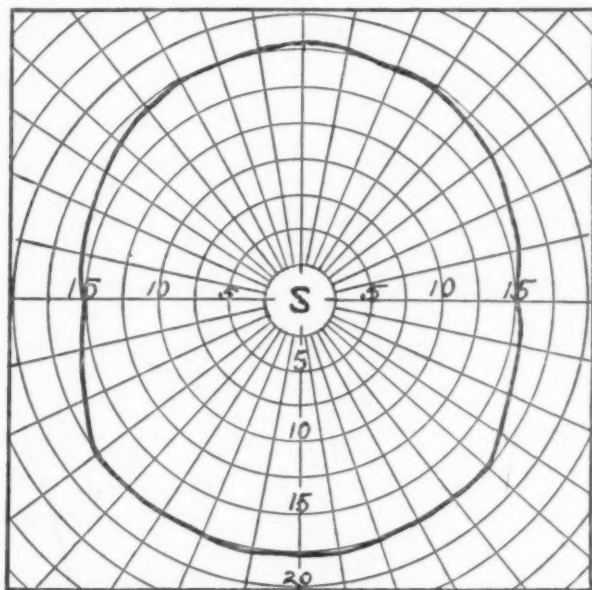


FIG. 1. Showing the distribution curve in the horizontal plane of a 50-watt carbon lamp, single oval filament—readings taken at 5 ft. radius; lamp operated at 6.8 horizontal cp.; watts per horizontal cp., 2.97.

a circle as to be scarcely detectable with the exception of a very small region in the plane of the filament. The curves for these lamps are appended in Figs. 1 and 2. In Fig. 3 is given also the curve for the ordinary type B Mazda lamp.¹ This curve shows more variation than the series lamp but it is so nearly uniform as to be considered circular for practical purposes. However, neither this nor the single oval filament carbon lamp have ever been used

¹ The determinations represented in these curves were made by the photometric laboratory of the General Electric Co., Schenectady, N. Y.

by us in connection with the method of photometry in question without some device to secure greater uniformity of distribution of light. In case a naked lamp were used at all it has always been of the series type, single-loop tip-anchored filament, and care has been taken to have the lamp set on the bar so that the light was taken at right angles to the plane of the filament or from the most uniform part of the curve. But even were a carbon lamp used and the arrow

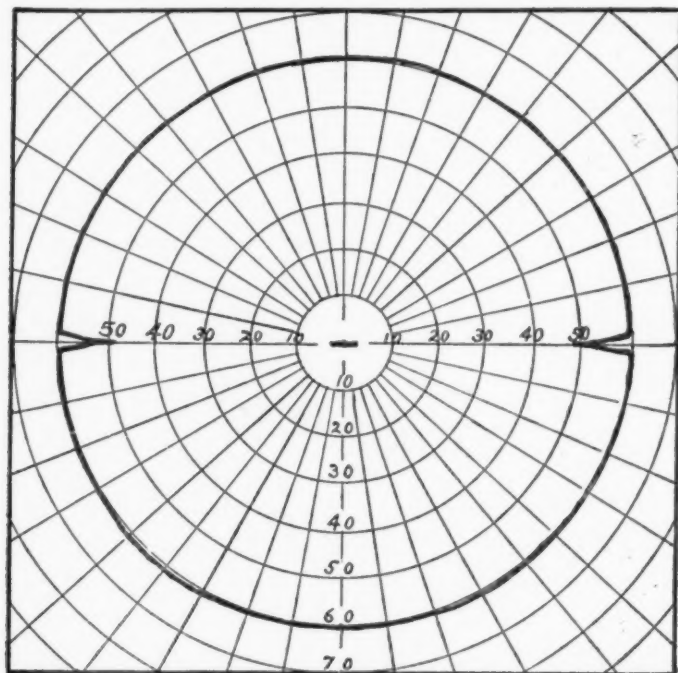


FIG. 2. Showing the distribution curve of a 60 cp. series Mazda lamp (clear), single loop tip anchored filament, 6.6 amps.—readings taken at 5 ft. radius; lamp operated at 60 horizontal cp.; watts per horizontal cp., 1.18.

or 'fiducial' mark scratched in a plane at right angles to the plane of the filament, the distribution would fall off so evenly on either side (see Fig. 1)¹ that the difference in the illumination of the stimulus patch and measuring disc, not exceeding 5.5° on either side, should be negligible.

¹ It should be noted that in making the cuts for the curves in Figs. 1 and 3 the true deviations from uniformity have been exaggerated by small but considerable amounts.

(b) So far as the question of uniformity of angular distribution of light is concerned, stress seems to be laid in the criticism on the equality of illumination of the stimulus patch and the measuring disc alone from the lights to be photometered. This is not at all in keeping with a correct interpretation of the method, for the photometric balance does not consist in the judgments of the actual amounts of light falling on the stimulus patch and measuring disc.

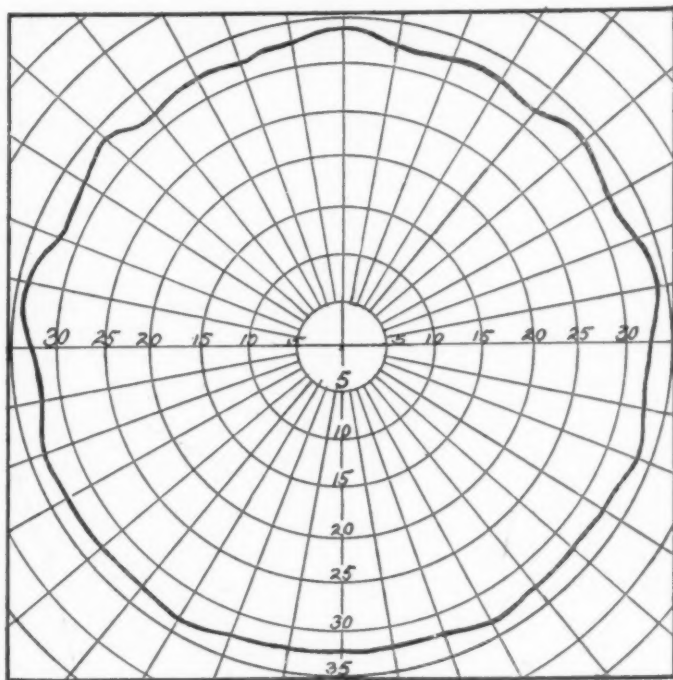


FIG. 3. Showing the distribution curve in the horizontal plane of a 40-watt G. E. Mazda lamp (clear), regular type small bulb, 110 volts—reading taken at 5 ft. radius; lamp operated at 32.5 horizontal cp.; watts per horizontal cp., 1.23.

The apparent brightness of the stimulus patch is, for example, the result of three factors: the actual amount of light falling on the stimulus patch, the amount falling on the surrounding screen (rather in both cases the amount reflected to the eye), and the physiological induction caused by the difference in the brightnesses of these two surfaces. Dr. Johnson, however, as indicated above, in considering the question of the distribution of the illumination and its probable effect on the results of the method, seems throughout his discussion

to take into account only the relative amounts of light received by the stimulus patch and the measuring disc, and in so doing shows a fundamental misunderstanding of the principle on which the method is based. The illumination of the field *surrounding* the stimulus patch is just as important as the illumination of the stimulus patch itself, for it is an equal factor in producing the induction and is, so far as any one knows, effective for induction up to the measuring disc; and there is, it is scarcely needful to point out, not an *angular separation of 15°* between this screen and the measuring disc. The important point is rather that there shall be no effective difference in the collective situation influencing the induction and its measurement for the standard and the comparison lamp. That is, although the two surfaces are compared in each judgment, the comparison of the two light sources is based on the results of two judgments, and if there is no difference in the collective situation influencing the two judgments, no injustice is done to the lights compared. If, therefore, we were considering with Dr. Johnson the relative illumination of stimulus patch and measuring disc to the exclusion of other factors, and to what degree this relative illumination is influenced by the distribution curve of the light source, the important item is not that there *is* an angular separation between them of a given number of degrees and a possible difference of illumination in consequence, but *how much this varies* for the position of the standard and comparison lamps on the photometer bar. For the nearest position of the standard lamp, the difference in the angular separation for the two lamps was 11° ; for the farthest position for the distances as given in the table, the difference would have been 4.5° . However, for the greater distances that would have been required for the standard lamp from the screen to establish a balance with the less intense colored lights, a part of the reduction was produced by sectorized discs, because in the form and set-up of apparatus employed for that work, distances of light from screen of 134-160 cm. (Table I., original article, p. 9) could not conveniently be attained owing to the angle of the shadow cast by the observer's head. This reduction was converted into terms of the law of squares to make the results comparable in the table with those obtained by the equality of brightness method. The actual setting of the lamp on the photometer bar for the greatest of these distances was 104 instead of 160 cm. The difference between the angular separation of stimulus patch and measuring disc was in this case, therefore, 7° . The actual range of variation of angular separation

of stimulus patch and measuring disc was thus only from 7° to 11° . There is, it is obvious, considerable difference between these values and the 15° with which Dr. Johnson confronts us. Furthermore, in the course of the original work we ran a series of check experiments to determine whether this difference in angular separation in case of the standard and comparison lights produced any significant error. That is, in these check experiments both lights were kept in the same position and the light for the more intense, the standard, was reduced by means of sectored discs very accurately cut from sheet aluminum, the open sectors of which were measured with a protractor provided with a Vernier scale reading to minutes. The results of these experiments are given in Table I.

TABLE I.

SHOWING A COMPARISON OF THE RESULTS OBTAINED FOR THE LIGHTS REPRESENTED IN THE ORIGINAL TABLE WHEN THE PHOTOMETRIC BALANCE WAS MADE (a) BY CHANGING THE SETTING OF THE LIGHTS ON THE PHOTOMETER BAR; AND (b) BY THE USE OF THE SECTORED DISC

Source of Colored Light	Color	Distance of White Light Giving Equality of Illumination, Cm.	Ratio of Candle-power. Color: White	Value of Open Sector Giving Equality of Illumination with Distance of White and Colored Lights Equal	Ratio of Candle-power. Color: White	Difference in Ratio	Difference in Per Cent. Candle-power
87 cp. 41 cm. distant from photometric screen.....	Red	66.6	0.379	137.5°	0.382	0.0030	0.785
52 cp. 38 cm. distant from photometric screen.....	Blue-green	59.5	0.4748	172.0	0.4778	0.0030	0.628
	Red	82.2	0.2137	77.75	0.2160	0.0023	1.06
13 cp. 38 cm. distant from photometric screen.....	Blue-green	70.5	0.2905	105.5	0.2931	0.0026	0.887
	Red	160.0	0.0564	20.5	0.05694	0.00054	0.948
	Blue-green	134.9	0.0793	28.85	0.08014	0.00079	0.985

Moreover, so far as inequalities of illumination of stimulus patch and measuring disc are concerned, we may point out that a naked lamp was not even used in the experiments the results of which are given in the original table. Partly because the colored light was secured by means of colored filters, and partly as a precaution against unevenness of illumination of stimulus patch, measuring disc, and surrounding field for a height and breadth sufficient for the purpose of the experiment, the light was placed in a lamp-house

(see original article, footnote p. 9).¹ This lamp-house was 24 cm. high, 14 cm. wide and 14 cm. deep. At the lower end of the lamp-house was an opening 5 cm. square through which the light passed to the screen. The lamp-house was lined with mat white paper so shaped as to round off the edges and corners and to give as much as possible in the lower part of the enclosure the effect of the segment of a sphere. No light passed directly from the lamp to the screen as the tip of the lamp was for the different lamps used from 2 to 9 cm. above the opening for the emission of the light. Owing to the high absorption of the Wrattan and Wainwright filters the light from the lamp used to establish a balance with that transmitted from the filters had to be greatly reduced at the opening of the lamp-house by means of colorless absorbing screens, which served further to diffuse the light. To determine whether or not any serious difference in the distribution of light to measuring disc and stimulus patch was present in case of this device, the light was photometered at stimulus patch and at measuring disc for each position of the lights on the bar. No difference could be detected for these two positions by the equality of brightness method. Also the distribution curve for the light coming through this opening was found to be circular through an angle greater than the 11° in question. Our statement in the original article then was correct that the equality of distance of the measuring disc and stimulus patch on either side of the photometer bar guaranteed that they receive equal illumination from the light source employed. It would also be true within any reasonable margin of error for the single loop filament series lamp set as described above (without a lamp house), and even for the single oval carbon filament within a margin of error quite acceptable for work in heterochromatic photometry.

3. In a later paragraph (p. 394) Dr. Johnson conveys the impression that we claim an agreement between the results of the new method and those of the equality of brightness method within

¹ The lamp-house is not shown in the photograph of apparatus given in the original article. The photograph was a part of the general description of the method and the apparatus that might be used with it. In making this photograph the apparatus was regrouped, the object being merely to show the type of bar used, the screen and the measuring disc. In this photograph it will also be noted that the apparatus was not even shown in the position in which it is used in making the determinations. The use of a lamp-house is mentioned in another part of the article, namely the part treating of the results that were given as a sample of what might be obtained with the method. In the first photographs that were made the lamp-house was included, but its size and position in the foreground made it appear so disproportionately large that it was decided to omit it and to give the photograph the general character mentioned above.

a fraction of one per cent.¹ Of this we have to say that no numerical value whatever was assigned to the agreement in the original article nor was any general statement made that would warrant the inference that we claimed an agreement within so small a margin. All that appears in the article in this connection is a very brief table of results containing no reference whatever to the point in question accompanied by an 8-line paragraph stating that the table is appended as a *sample* of the results obtained, that the results are averages from 25 determinations, etc. It is the custom in photometry when a numerical expression is made of agreements, mean deviations, etc., to give these in per cent. illumination or per cent. candlepower. When this is done for the table in question, the agreement shown by the data given falls within 1.5 per cent. instead of 'within a fraction of 1 per cent.' as is stated by Dr. Johnson. And this it will be remembered, is an agreement in the average. When the individual determinations are compared, the deviations reach values of the order of + 10 and - 12 per cent. Some idea of this may be had from an inspection of the per cent. mean variations appearing in the table for the results obtained by the equality of brightness method. Thus it will be seen that the actual closeness of agreement of results is not surprising. It has been made to appear so only by our critic's method of presentation.

¹ On p. 393 Dr. Johnson says: "The authors do not describe their mode of procedure in making their measurements by the method of direct comparison. I assume, therefore, Under these conditions and working with the lamps beyond certain minimal distances from the photometer head, the luminous intensities of the compared sources would be *inversely* [italics ours] as the squares of their distances from the photometer screen at valid settings for equality of brightness on the two halves of the photometer field." We did not suppose that in an article on photometry it was necessary to give a description of the equality of brightness method over 100 years after its principles were laid down for all time (Pierre Bouguer, 1760, and Sir Benjamin Thompson, Count of Rumford, 1793). However, we do wish to say now that Dr. Johnson has raised the question that we conformed to all that is essential in his very elementary directions with the exception that we chose rather to follow the custom to which we know of no exception either in practice or recommendation, of calculating the luminous intensities of the light sources on the basis of the *direct* squares of the distances of these light sources from the photometer head, instead of the *inverse* squares. In replying to an advanced criticism on photometric method, one should not have to point out that the law of inverse squares applies to the intensity of illumination at different distances from a given source; while the converse of this relation, namely, the direct squares, applies to the comparative intensities of two sources which produce equal illumination on a given screen or photometer head. That is, the former is used in the computations of intensity of illumination: foot-candles, meter-candles, etc.; and the latter in the computation of the relative intensities of light sources: candlepower, lamberts, millilamberts, etc.

4. Also on p. 394 Dr. Johnson presents a table in which it is represented that the measuring disc in the work for which our table of results was submitted was 3 cm. nearer to the observer than the plane of the screen containing the stimulus patch. Applying the law of inverse squares he demonstrates that the illumination of the stimulus patch and measuring disc was in case of each light source unequal. Since the colored lights were all nearer the screen and measuring disc than the standard white light in proportions varying from $41/59$ to $38/160$ (actually $41/59$ to $38/106$ because, as stated earlier, a sectorized disc was used for the lights requiring the greater distance of setting from the screen), the 3 cm. caused a greater difference between the illumination of the measuring disc than of the stimulus patch for the colored lights than for the white light by percentages ranging from 5.4 to 15.5. From the showing of this table without further inquiry into causes,¹ it was concluded that 'the authors' procedure in making the settings was faulty,' the 'method is insensitive' and that the evidence of agreement of the two methods is 'spurious,' for the explanation of which latter point there seems to have been no hypothesis worthy of mention but that the settings of one method were biased by a knowledge of the settings of the other—a smashing and uncompromising arraignment truly! However, we beg in passing to say a word of this table ourselves. In the first place, as a matter of only minor consequence to the present discussion, we wish to point out that in all of the computations given by Dr. Johnson of the deviations in per cent. from proportionality of illumination of stimulus patch and measuring disc, errors have been made, and that in 5 out of a total of 6 cases appearing in his table these errors have ranged from 1.8 to 11 per cent. of the correct value, with a leaning in some of the most important cases towards the advantage of the critic. This, we may be pardoned for noting, is under the circumstances somewhat surprising, and is of value perhaps chiefly in demonstrating that it is possible for mistakes to occur even in a critique levelled at the accuracy of the work of others without furnishing a justification for the impugning of motives and integrities. And secondly we wish to state that, as might have been suspected by our critic himself,¹ the 3 cm.

¹ The above statement is made for the following reasons. (a) It is obvious on *a priori* grounds to one having even the least rudimentary knowledge of the principles on which photometry is based, that a just balance could not be established between the colored and white lights involving so wide a difference in setting on the bar if the measuring disc was 3 cm. in front of the photometer screen. And (b) even an approximate set-up of the apparatus with the lights in position demonstrates at a glance that

was a typographical error. In the original data still in our possession, the distance of the measuring disc from the screen is given as .3 cm.¹ When the law of inverse squares is applied to this, the discrepancy of illumination of stimulus patch and measuring disc for the distances used by Dr. Johnson in his computations ranges from .464 to 1.22 per cent., and for the actual distances used, from .464 to 1.03 per cent.—an amount which the experienced photometrist will, we think, grant is relatively negligible among the much greater sources of error present in heterochromatic photometry.

We have, however, been sufficiently curious to know what results would be obtained with the measuring disc placed 3 cm. in front of the screen to repeat the work represented in the original table for the four highest intensities with this change in the set-up. Differences from the results quoted in the original tables—also, as it happens for the cases tested, the amount of deviation from agreement with the equality of brightness results—ranged from 13.5 to 25 per cent. when the determination was begun with the weaker light, and from 18.6 to 29 per cent. when the determination was begun with the stronger light.² These figures indicate that rather than being remarkable for its insensitivity, as is charged by Dr. Johnson on the basis of too narrow a consideration of possibilities and apparently no first-hand knowledge whatever of the facts in question, the method shows by still another test a very high degree of sensitivity.

5. The error in our critic's final conclusion (pp. 395-6) should by this time be so obvious as to need no comment. We will, therefore, rest our case so far as we recognize that a case has existed, until space can be had for a further presentation of results. In this regard it is hardly necessary to mention that we do not consider, the conditions produced are not compatible with the principles on which the method of making the balance is based. For example, when illuminated directly from the lamp on the bar a sharp shadow is cast by the disc on the screen, which is plainly in the view of the observer at the angle at which the observation is made. This is the equivalent of surrounding the disc with a black band which varies in width as the position of the lamp on the bar is changed. This is obviously not permissible. In fact the error is of a kind which is usually handled in a note of inquiry to the authors.

¹ Also there are, we might mention, a number of witnesses to the set-up of the apparatus used by us in the work on heterochromatic photometry.

² On account of the limited space allowed, an explanation of why such excessive deviations are obtained with this incorrect set-up will have to be deferred until later work; also the very obvious explanation of why a greater distance of measuring disc from screen was permissible, in fact of advantage, in the work in which the method was used to detect changes in the diffuse illumination of an optics-room (PSYCHOL. BULL., 1913, 10, p. 371) than when it was applied to the rating of lights on a bar.

as our critic seems to have thought, that a place has been won for our method among those hoary and worn with service on the basis of a single sample table appended to a preliminary description of method and apparatus and representing the results of only one observer for two colors and only six of the possible settings on the photometer bar.

NOTE.—Dr. Johnson mentioned the use of a rotator to equalize the light radiation in different directions; also the deviations found by Wright from Lambert's law of reflection for mat surfaces. Since neither of these points was raised in the original article, it might be inferred that they were not known and taken into account by the authors. It will probably not be prejudicial to either side of the case to mention here that one of the writers supervised the construction of his first lamp rotator for work in photometry in 1901 while a teacher of physics, and is well acquainted with the uses and need of a rotator. Also in 1903 while a graduate student of physics he was assigned a study of the reflection from mat surfaces as a problem for investigation, the object being to continue along the lines mapped out by Wright. Both from his reading and instruction with regard to the work of Wright and others, however, he is totally unable to concur in a single comment that Dr. Johnson has made on the subject of diffuse reflection in the footnote on p. 392. Dr. Johnson says: "Another source of error which the authors appear not to have taken into account may be worthy of mention. The angles at which the light was diffusely reflected into the eye from the stimulus patch and the disc at the fixation point were not the same. The *percentage* of incident light reflected into the eye would have been different, therefore, even if the two surfaces had been of the same material. Furthermore, the difference in *percentage* of incident light reflected in the direction of the eye is not constant for any two positions of the source. Cf. Wright, H. R., 'Photometry of the Diffuse Reflection of Light on Matt Surfaces,' *Philos. Trans.*, 1900, 49, Ser. 5, pp. 199-216." Of the sentences quoted the second is the only one that can be said to be true. The angle of emission ϵ from the stimulus patch in relation to the eye was approximately 0° ; while for the measuring disc it was 25° . The reflection, therefore, in the direction of the eye from a given point or unit surface in the area fixated of the measuring disc was less than that from the stimulus patch by an amount equal to the cosine of 25° . Dr. Johnson, however, neglects to take into account in considering the case presented by our method that the observation is not confined to a single point or unit of area and that the area of surface viewed increases as the secant (the reciprocal of the cosine) of the angle at which the surface is viewed measured from the normal. That is, the increase of the area viewed just compensates for the lessened amount of reflection from unit area. Nutting, for example, says: "A red hot metal plate is of the same brightness viewed at any angle since the foreshortening of the area just compensates for the variation in the radiation from a given area. Lambert's law holds for mat surfaces for both emitted and reflected radiation." Even the author referred to by our critic, in discussing the two possible methods of making the photometric determination in his investigation of the reflection from mat surfaces, says in effect the same thing (cf. Wright, p. 205), so without exception does every other author after whom we have read. Therefore when two mat surfaces are observed whose areas are not limited, the apparent brightness of these surfaces is the same for different angles of observation provided that the angle of incidence and amount of incident light are the same for both surfaces as was the case for the stimulus patch and measuring disc in our work for any one setting of the light on the bar; for

although the reflection from unit area decreases as the cosine of the angle of reflection, the area from which the eye receives its light increases as the secant of the same angle; from which it follows that the amount of light entering or reflected in the direction of the eye is independent of the angle at which the surface is viewed.

It is obvious, then, that Dr. Johnson's statement that the *percentage* of incident light reflected in the direction of the eye would have been different, even if the two surfaces had been of the same material, is not true. From this it is equally obvious that his next statement also is not true, namely, that the *difference* in the *percentage* of incident light reflected in the direction of the eye is not constant for any two positions of the source, for as shown above there is no difference in the *percentage* of incident light reflected to the eye from the two surfaces for any given setting of the light on the bar. In other words, the possible bearing of Lambert's law and Wright's results with regard to this law, is not what Dr. Johnson has stated it to be. Just what this bearing is will be discussed further on in this note. What we wish to do at this point is to show that even if it were true that the percentage of incident light reflected to the eye were different for any one setting of the light on the photometer bar, this would make no difference whatever in the results obtained by our method. That is, if less light were reflected to the eye from the measuring disc than from the stimulus patch for the first light set upon the bar, it would mean merely that the coefficient of reflection of the measuring disc would have to be reduced by a corresponding amount to obtain the match. Then when the comparison light was placed on the bar and its distance adjusted until as much light was given to the screen as was received from the first light, the stimulus patch and measuring disc would again match, for neither the difference in angle of reflection to the eye nor the reflection coefficients would have been changed. Dr. Johnson's point, granting its verity, would have application only if the stimulus patch were illuminated alone by one of the lights and the measuring disc by the other and the method of balancing consisted in bringing these two surfaces to equality—then it would be necessary that each reflect to the eye the same percentage of the light received by it; but the point is clearly quite irrelevant to the method described by us in which the two surfaces are illuminated for each judgment by only one of the lights, and the balance consists in so adjusting the distance of the two lights in the successive judgments that the match for the one based on the amount of induction produced at the stimulus patch holds also for the other. In this case it is important only that the physical situation and other factors be kept the same for both judgments—not that they be equal each to each for the single judgment—for the balance is based on the principle that if all the factors are kept constant the amount of induction at the stimulus patch will always be the same when the same amounts of light are received on the screen. It is obvious also that the same considerations are true with regard to the materials forming the stimulus patch and measuring disc. Moreover, with reference to this point, it may also be said that there was, as a matter of fact, very little difference in the materials forming the two surfaces; for one sector of the measuring disc was identical with the stimulus patch and the other sector was a darker gray of the same series of papers (Hering's series of standard grays).

In concluding our comments on this footnote which has revealed so much of our critic's point of view, we will indicate briefly and only in a general way the relation of Lambert's law of reflection from mat surfaces and Wright's findings with regard to this law to the practical working of our method. As already shown, Dr. Johnson's criticism was based both on an erroneous understanding of this law as applied to the making of the photometric judgment by any method whatsoever and on a wrong conception of

the principles of the method criticized. Our actual chance of error in terms of Lambert's law is that the *angle of incidence* (Johnson's 'difference in *angle of reflection*' has nothing whatever to do with photometry from mat surfaces) on the stimulus patch and its surrounding field is different for the light from the standard and comparison lamps when they are of different intensities and a different setting on the bar is required to establish the photometric balance. That is, according to Lambert's law the intensity of the illumination of the stimulus patch and its surrounding field is proportional to the cosine of the angle of incidence (the cosine i).

Now considering for the sake of simplicity the stimulus patch alone, the variation in the cosine of the angle of the incident light for the entire range covered in the work criticized from the least to the greatest distance of the source of light from the screen, falls within 1 per cent. While this would mean only a comparatively slight difference in the induction situation from the lights compared, we have from the beginning in our own thinking frankly faced it as a small source of error in case the reductions of the light on the screen are produced by changing the position of the lamps on the bar. However, it would not enter in at all, as will be readily seen, if the reduction of light is produced by means of a sectored disc or any device: absorbing screen, Nicol's prism, grating, etc., which does not change the distance of the source of light from the screen and, therefore, the angle of incidence of the light on the stimulus patch. In this regard it should be remembered too that our photometer is no more at fault in physical principle than the equality of brightness photometer after Rumford as ordinarily constructed, in which also the angle of incidence is changed with a change of the position of the light on the bar—not so much at fault perhaps, for compensating factors operate in our method of getting the balance which are not present in the Rumford method. The relation of Wright's results to the situation described here is that he found that there are certain small deviations from the law of the cosine i as the angle of incidence is changed. Now just how great the chance of error is in our method from the law of the cosine i considered in relation with the results of Wright it is utterly impossible to estimate with any acceptable degree of precision from the principles involved for the following reasons: (a) The surrounding field as well as the stimulus patch must be taken into account in applying the law of cosines. The difference in the angle of incidence for the different points in this field vary for any two positions of the light on the bar—towards zero as a limit, for example, for the points between the stimulus patch and the end of the bar, and differently in other directions. (b) The effect is not direct but operates through induction, the quantitative relations of which are not definitely known. And (c) Wright apparently considered it worth while to make no change of angle of incidence smaller than 20° , while the entire range of variation of this angle in our work from greatest to least distance of lamp from screen was for the colorless light 2° and for both the colored and colorless lights 5° .

Rather, therefore, than indulge in bootless speculation in regard to the possibilities of error from these sources, it is obviously much more to the point to get some empirical measure of their effective importance. The effective importance of this factor along with others not mentioned by Dr. Johnson may be checked up (a) by a comparison of results in the average with those obtained by the equality of brightness method (see table in original article, p. 9); and (b) still more definitely and directly by comparing the results obtained by the method when the reductions of the light on the screen are produced by changing the distances of the sources from the screen and when the distance of the source and, therefore, the angle of incidence of the light is kept constant and the reductions are made by means of a sectored disc (see Table I. of this discussion).

Even had these comparisons not been made, the probable relative unimportance of these sources of error as compared with the high variable error obtained for one or any small number of determinations by the equality of brightness method, should, we think, be obvious to all who have a working familiarity with the latter method in heterochromatic photometry. On the point of sureness of principle, moreover, it is instructive to compare the agreements of the induction and equality of brightness methods shown in the tables referred to above with those obtained for the equality of brightness and flicker methods, for example, for lights presenting the same amount of color difference.

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[The above discussion, which exceeds our usual limits, has been accepted by the Editors in order that the authors might have ample opportunity to clear up the points raised in Dr. Johnson's NOTE. The questions at issue are so specialized and technical that we believe it unprofitable to continue the discussion in the pages of the REVIEW. A committee of experts acceptable to both parties may be suggested as the best means of settling any differences which remain between the writers and their critic.—THE EDITORS.]

THE STANFORD (1915) AND THE VINELAND (1911) REVISIONS OF THE BINET SCALE

A brief analysis of the Stanford and Vineland revisions is here attempted in order to indicate a few of the chief points of difference.

A hasty review of the Stanford revised scale gives one the impression that it is much more difficult throughout.¹ The extension of the scale to age 19½ (superior adult) is certainly a commendable advance.

The scale begins at age three and each age contains six tests, in addition to from one to three alternate tests for each year up to age ten. The placing of six tests in each year permits assigning a two months' value to each test. There are no tests for age 11, the only other ages listed being 12, 14, 16, and 18. Since there are no tests for age 11, the eight tests in age 12 are each made to count toward three months mental age, yielding a total value of two years. By this means tests for 14, 16 and 18, six per year, are made to cover without break the range of mental development from 12 to 19½.

The Vineland revision consists of 49 tests for the ages between 3 and 12. Covering the same period in the Stanford revision they number 56, with 13 additional questions which may be used as alternates.

In the following tables the evolution through which the Vineland revision passes is indicated test for test. Tests not in the Vineland (1911) are printed in italics.

VINELAND (1911)		STANFORD (1915)	
		Age—Test	
Age III.	Test 1—Pointg. eyes, etc....remains..becomes.....	III-1	
	" 2—Rpts. 6 syll.....remains..becomes.....	III-6	
	" 3—Rpts. 2 nos.....omitted		
	" 4—Enumer. of pic....remains..becomes.....	III-3	
	" 5—Knows name.....remains..becomes.....	III-5	
		Vineld. IV-1 becomes....	III-4
		" IV-2 "III-2
		" IV-3 "III-A. 1

¹ See foot-note, p. 179.

	VINELAND (1911)	STANFORD (1915)
Age IV.	Test 1—Knows sex..shifted to III-4	
	“ 2—Recog. key, etc..... “ “ III-2	
	“ 3—Rpts. 3 nos.. “ “ III-A. 1	
	“ 4—Compr. lines.....remains..becomes.....IV-1	
		<i>Klmns. Form Discr....</i> = IV-2
		<i>Vineld. V-4.....</i> = IV-3
		“ <i>V-2.....</i> = IV-4
		<i>Binet Compr. 1 deg....</i> = IV-5
		<i>Stanfd. 4 digits.....</i> = IV-6
		<i>Vineld. V-3.....</i> = IV-A. 1
Age V.	Test 1—Compares wts.....remains..becomes.....V-1	
	“ 2—Copies sq....shifted to IV-4	
	“ 3—Rpts. 11 syll. “ “ IV-A. 1	
	“ 4—Counts 4c... “ “ IV-3	
	“ 5—“Patience”.....remains..becomes.....V-5	
		<i>Vineld. VII-5.....</i> = V-2
		“ <i>VI-5.....</i> = V-3
		“ <i>VI-2.....</i> = V-4
		“ <i>VI-3.....</i> = V-6
		<i>Binet, age.....</i> = V-A. 1
Age VI.	Test 1—A. M.—P. M.....remains..becomes.....VI-A. 1	
	“ 2—Definit’ns, use..shifted to V-4	
	“ 3—3 direct’ns..... “ “ V-6	
	“ 4—R. hand, L. ear.....remains..becomes.....VI-1	
	“ 5—Aesthet. Comp..shifted to V-3	
		<i>Vineld. VII-3.....</i> = VI-2
		“ <i>VII-1.....</i> = VI-3
		“ <i>X-4 (1st ser.)</i> = VI-4
		“ <i>X-1 (part)....</i> = VI-5
		<i>Stanfd. 16-18 syll.....</i> = VI-6
Age VII.	Test 1—Counts 13c...shifted to VI-3	
	“ 2—Descr. pic.....remains..becomes..... = VII-2	
	“ 3—Unfin. pic....shifted to VI-2	
	“ 4—Copies Diamd.....remains..becomes..... = VII-6	
	“ 5—Colors.....shifted to V-2	
		<i>Binet, fingers.....</i> = VII-1
		<i>Vineld. VIII-5.....</i> = VII-3
		<i>Stanfd. bow-knot.....</i> = VII-4
		<i>Vineld. VIII-1.....</i> = VII-5
		“ <i>VIII-3.....</i> = VII-A. 1
		<i>Stanfd. rpt. 3 no. bkwd.</i> = VII-A. 2

In age 5, two tests remain and three are shifted to age 4, being too easy for children of five.

In age 6, two tests remain and three are shifted to age 5.

In age 7, two tests remain, two are shifted to age 6, and one to age 5.

In age 8, one test remains, three are shifted to age 7, and one to age 9.

In age 9, four tests remain and one is shifted to age 8.

In age 10, two tests and a portion of a third remains, one test is shifted to age 9, one part each of two tests is shifted to age 6, and one part each of two tests is shifted to age 8.

In age 11, all the tests are shifted, there being no corresponding 11-year group in the Stanford revision: two are shifted to age 9, two to age 10, and one to age 12.

In age 12, one test remains, one is omitted, and three are shifted: one to age 10, and two to age 14.

In age 15, one test is omitted and the other three are shifted as follows: one to age 12, one to age 14, and one to age 16.

Of the "Adult" tests, one is omitted, one becomes a test for age 14, one a test for age 16, and two are tests in age 18.

The above changes are indicated in the following table:

TESTS

Age	No Change	Omitted	Shifted						Total
			Earlier				Later		
			1	2	3	4	1	2	
3	4	1							
4	1		3						
5	2		3						
6	2		3						
7	2		2	1					
8	1		3				1		
9	4		1						
10	2½	½	1	½		½			
11	—		2	2			1		
12	1	1		1				2	
15	—	1	1		1		1		
Adult ¹	3	1		1					
Total.....	22½	4½	19	5½	1	½	3	2	58
Percent....	38.5	7.5	32.8	9.8	1.7	1.1	5.2	3.4	100.0

¹ (16-18)?

It will be observed that between 3 and 7 years twelve tests have been removed to earlier years, and no tests to later years.

These changes will dispose of the criticism that the lower end of the scale is too easy. But between 8 and 12 years 14½ tests have been removed to earlier years, and only five tests to later years, thus making the scale still more difficult at its upper end.¹

38.5 percent of the scale remains unchanged. 7.5 percent of the tests in the Vineland revision are omitted. 45.4 percent of the tests are found too easy for their respective ages and are shifted to earlier years, 32.8 percent being placed in the next earlier age. 7.6 percent of the tests are found too difficult for their respective ages and are consequently placed in later years.

No test is placed more than four years below its original position. No test is placed more than two years above its original position.

It might be well to keep in mind that the tests appearing only in the Stanford revision have not been considered in the above tabulation and in the summarization of the test data. For that reason some of the statements made need not be regarded as seriously critical.

Using the Stanford revision, Terman and his collaborators found that (a) by using the intelligence quotient one can transform the 'age grade scale' into a 'point scale' automatically, should one prefer expressing the development of intelligence in that manner. "As such it would seem to be greatly superior to the Yerkes-Bridges scale, for it includes a much larger number of tests and its points have definite meaning and equal value." (b) Sex-differences are found to be so small as to be negligible for practical purposes. (c) The younger the children the greater the influence of social status on intelligence.

The Stanford revision is to be welcomed in its effort toward a scale free from those objections which are still being quixotically hurled against it.

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¹ The following article, however, "L. M. Terman and H. E. Knollin: Some Problems Relating to the Detection of Borderline Cases of Mental Deficiency" *J. Psycho-Asthen.* 1915, 20: 1-15, coming to the notice of the writer after the above was in type, shows the reverse to be true. Tabulating the reactions of borderline subjects (mental ages by the Stanford Revision between 12 and 14, —104 adults) they found that by the Vineland Revision, weighted for tests above 12, the median age for these subjects was reduced as much as one and one-half years, and with the tests unweighted the reduction was greater, namely two years. It ought also be mentioned, in this connection, that the procedure and scoring of quite a number of tests have been changed in the Stanford Revision. Consequently a *strict* analysis of test displacement must take these facts into consideration. Change of procedure or scoring may so modify the statistical data obtained for a test as to warrant its transfer to some lower year without necessarily increasing the difficulty of the scale at that particular point.





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